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I. "PROGRESS,"

THE ALLEGED DISTINCTION BETWEEN MAN AND BRUTE.

IS man essentially progressive? Are all other animals as essentially stationary? The public opinion of these latter days and a number of eminent writers answer both these questions in the affirmative, trusting to find here the long-sought "great gulf."

To man they ascribe a threefold movement—of the individual, of the community, and of the species. To brutes they ascribe immobility. Disregarding the charge of "cynicism" which may perhaps be brought against us, we shall examine a portion at least of these assertions. We shall make bold to ask whether it is really true, as so often asserted, that man is from the cradle to the grave a "progressive being," still able and willing to learn, and to improve even to the very last? Do his views as he grows older become continually freer and wider, his insights deeper, and his prejudices feebler? The notion can scarcely be fairly and fully stated without revealing its utter absurdity. It is one of those errors which men assert in general terms, and yet deny daily in words as well as actions. Every observer of human nature, every man of experience, admits that the mind, like the body, up to a certain period increases in its powers, and afterwards gradually declines. Like the body, it becomes first less flexible, and then feebler. Figuratively speaking it is ossified. The epoch of this change varies not a little in different individuals. Yet in almost every man there comes a time when he ceases to grow mentally just as he has ceased to grow corporeally—when he no longer looks forward; losing at once the power and the desire to observe new facts and evolve new theories, or even to recognise them if discovered by others. He becomes stagnant, and instead of the bold "plus ultra" of his prime he feebly murmurs "Rest and be thankful."

It is stated that not a single physician who had reached the age of forty, prior to the announcement of Harvey's discovery of the circulation of the blood, could ever be brought to recognise it as correct. World-betterers of all shades, moral and religious reformers who are labouring at the difficult task of improving man, continually bear testimony that "it is of little use attempting to elevate or amend the adults; we must turn our attention mainly to the children." In so saying they substantially admit our position. There are, indeed, men of vast and highly-cultivated intelligence, who, up to an advanced age, and even to the very close of their days, seem to go on thinking and discovering, to long for new truths, and to weigh every novel theory with candour and impartiality. But these are the very exceptions which prove the rule. Their biographers do not fail to point out this persistent energy of the mind as a proof of their exceptional nature and of their superiority to ordinary men.

Nor must we forget that many of the discoveries and other great works, emanating apparently from men far advanced in life, have in reality been wrought out in their earlier days, though the publication of the results, especially in a popular form, has been delayed. The power of expression, too, as a rule, survives by many years the power of origination.

But what of the normal average man? Is he not from the middle of life, and often indeed from an earlier date, a mere bundle of habits and prejudices? His interest in anything not immediately relating to self—where it ever existed—has dwindled. His mind has lost its suggestiveness. He shrinks from change of place, scene, and occupation. He dislikes new associates. He suspects new views, and imputes motives to their promulgators. With him, just as with the lower animals, no further mental growth is possible, however long he may happen to survive.

The truth here put forward has recently received full demonstration—if such were needed—at the hands of an American man of science, Dr. G. M. Baird. In his treatise* he shows that 70 per cent of the work of the world is done before 45, and 80 per cent before 50. "The golden decade of a man's life (the interval between his thirtieth and fortieth year) alone represents nearly one-third of the work of the world. The advantage of the period from 20 to 30 over that from 50 to 60 is very striking, and will cause surprise."

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* **Legal Responsibility in Old Age.**

In short, from a careful biographical examination, it fully appears that in the individual man—so far at least as his present life is concerned—there is no indefinite and continued progress. His powers of body and mind are developed together, and simultaneously—or nearly so—they decline. The same law applies to him as to the lower animals, and, if we take into account the different term of life of each species, all have their "golden epoch" about the same part of their career.

To man, then, brutes exhibit no well-marked contrast, but a decided similarity. In the earlier part of life they are, like ourselves, capable of progress; they observe, learn, and retain facts, and to a certain extent they draw conclusions from such observations. By-and-bye their faculties are blunted, and, like man, they become stationary. That in them stagnation may set in somewhat earlier than it does in civilised man is not impossible. But this is a mere difference of degree, not of kind. It is very easy for the rhetorician to exclaim—"The brute soon attains a degree of perfection which he can never surpass, and were he to live a thousand years he would still be the same." This is no less exact a portrait of the human species than of the gorilla, the dog, or the ant. As far, then, as individual progress is concerned, man and beast differ merely in degree.

There is also among mankind a national or tribal progress distinct from that of the individual and that of the species, but, like them both, not unlimited in its extent and duration. Historians tell us how a small community has, if unchecked by hostile circumstances, grown up into a great empire, and how, again, it has declined and come to ruin. For such national decay they assign a variety of causes, according to their political and social prepossessions. In one case the calamity is ascribed to luxury; in another, to the toleration of slavery or of usury; in others, to the importation of foreign-grown corn, to the non-recognition of "woman's rights," or to the existence of vivisection, and other the like springs, which to examine would be a departure from our purpose. But whatever share of blame may attach to such causes, we should seek for the main source of the evil in the decay of that which in a nation corresponds to vitality in the individual,—that is to say, of the so-called "tribal instinct." When this instinct gives place either to avowed selfishness or to cosmopolitanism—and the two differ really more in seeming than in being—the doom of the nation is sealed, despite alike of its resources and its virtues.

Among those animal species which live in organised communities or nations the very same phenomenon occurs; increase and rise are followed by decline and fall. Every ant-hill might have its Gibbon. This fact has been fully established by the illustrious French chemist M. Berthelot, who for the last quarter of a century has devoted much of his leisure to a study of the manners of ants.* He noticed in particular a very prosperous ant-hill, which had established roads in all directions of more than 100 metres in length. After about ten years it sent out a colony which established itself at the root of a young oak at some little distance, and though feeble in the outset increased year by year, and passed safely through a very critical epoch, the periodical felling of the wood. The observations of M. Berthelot were temporarily interrupted by the war. On resuming his studies he found, to his surprise, the colony in full prosperity, but the mother-city declining. Its population had lessened, and continued so to do, and the survivors neglected their habitations. The former colony now takes the lead, and its new buildings and well-kept roads contrast strangely with the crumbling structures of the old formicary, which, however, is still inhabited.

We have ourselves observed a very similar case among rooks. An old and populous rookery from no apparent cause dwindled away, whilst a colony which it had founded in its early days gradually surpassed the mother-city, and became in turn the rook-capital of the district. Surely, then, we must admit that the cities and nations of brutes, like those of mankind, have their origin, their progress, and their decline. Here therefore, again, instead of contrast we find resemblance.

We turn now to what is in popular literature more generally known as "progress"—the real or fancied superiority of every generation of mankind over the foregoing. But this very superiority, though assumed in after-dinner speeches and in songs of the "good time coming" type, is a matter of most grave question. Even as regards knowledge and power, the advance which some claim as a characteristic feature of humanity is effected by exceptional individuals who arise in certain races under favourable circumstances only, and is quite compatible with long intervals of immobility, and even of decline. Can a property so rare and fluctuating be made an essential point in any definition of man, or can it be used to establish a "great gulf" between him and the brute creation?

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* Correspondance Scientifique, Oct. 1, 1878.

Further, it is nowise proved that the lower animals are literally incapable of progress. Our acquaintance with the habits and powers of most brute species, incomplete even at the present day, is far too recent to justify such an assumption. On the other hand, cases are not wanting which can scarcely be regarded in any other light than as inventions made, *e.g.*, by ants. Thus, from a work recently noticed in the "Quarterly Journal of Science,"* we quote the following incident recorded by Prof. Gredler, of Boston:—"One of his colleagues had for months been in the habit of sprinkling pounded sugar on the sill of his window, for a train of ants which passed in constant procession from the garden to the window. One day he took it into his head to put the pounded sugar into a vessel which he fastened with a string to the transom of the window, and, in order that his long-petted insects might have information of the supply suspended above, a number of the same set of ants were placed with the sugar in the vessel. These busy creatures forthwith seized on the particles of sugar, and soon discovering the only way open to them—viz., up the string, over the transom, and down the window-frame—rejoined their fellows on the sill, whence they could resume the old route down the wall into the garden. Before long the route over the new track, from the sill to the sugar by the window-frame, transom, and string, was completely established, and so passed a day or two without anything new. Then one morning it was noticed that the ants were stopping at their old place, the window-sill, and again getting sugar there. Not a single individual any longer traversed the path that led thence to the sugar above. This was not because the store above had been exhausted, but because some dozen little fellows were working away vigorously and incessantly up aloft in the vessel, dragging the sugar-crumbs to its edge, and throwing them down to their comrades below on the sill—a sill which, with their limited range of vision, they could not possibly see."

This is indisputably an instance of invention, and as all man's progress in the industrial arts, in power, whether destructive or constructive, is ultimately due to invention, the alleged stationariness of the lower animals as compared with man can no longer be maintained.

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* Flowers and their Unbidden Guests, p. 21.

II. MATTER DEAD.

"Elementa sunt facta de Yle."—ROGER BACON.

LET us suppose Roger Bacon to be a good representative of the philosophy that matter is one, and John Dalton as representing belief in the primordial diversity of atoms, and let each have a modern mouthpiece. We can imagine the following :—

Roger Bacon—I have read in a German book something like this ; Give me an atom, and I make the world, life and thought included. This was not writing of the highest class there, but it was from observing the tone of the writing of some of the most talented men, and I certainly think it not quite unfair to draw such conclusions as the writer drew after imbibing for truth the thoughts of the most prominent men. Have I not seen, also, in the writings of one of the men considered first in rank, and certainly by himself as first in importance, that the finest feelings of the soul are all dependent on a certain fluid which we are left to suppose produces them.

To the first observation there is a short reply, and if any man founds his philosophy on such a supposed knowledge of the action of atoms he can gain confidence only from the most ignorant or the most illogical. We do not know how any atom acts, and the assertion is simply akin to the sayings of Baron Munchausen.

If to the second observation any one gives trust, he may be correct in a sense, as we may say no conscious thought occurs in man without water ; but if any man will say that he knows why any certain substance, liquid or solid, acts in producing thought, he is a visionary, and does not belong to the class of sound scientific men : him also I put on my shelves with the boasting Baron. And yet how many men there are who suppose that these things are actually known, whilst there are some who speak amongst us as if they knew them. Men assert the power of matter who have not to themselves clearly defined what matter is. Most men, even scientific men, speak as if it were only the substances known to us in chemistry as "the elements." Some will allow both elements and forces, or ponderable and imponderable matter, the imponderable acting as forces; but it is probably the opinion most prevalent that matter and force are one. To use the words of one of the most recent volumes, by a scientific man of careful thought,—"Wherever matter exists, energy (either potential or active) also exists (or matter is the seat of energy)—is another truth of the widest kind, based upon universal experience, and is inferred from the fact that energy has never been observed except in connection with matter; *i.e.*, in connection with that which possesses weight."

John Dalton—We cast the seeds into the earth, and they grow, collecting matter willingly, whilst matter readily flows to them. A tree grows, and we throw it into the fire; it acts with violence, and matter again collects about it and carries off all that had been so laboriously brought together. Surely matter is alive.

Is not the world in a constant state of motion itself, whilst its surface is restless, and much of it extremely agitated? Surely, too, every element is vigorous, scarcely any to be found in freedom, all attached servilely to some other, and by constant changings keeping up the proof of vitality. This world is far apart from other worlds, and yet it keeps in constant communication with them and shows its sympathy, which can only exist because of its life.

The elements in many cases cling violently to each other, and combination is accompanied by burning heat, which heat again goes to set other bodies in motion. Matter is therefore not only powerful, but it is a reservoir of power.

So active is matter that if it is freed from the violence of cohesive enchainment, as in a state of gas, every particle raves continually, and is in eternal wildest motion as constantly as the moving planets, if not so carefully regulated in every step. Here is eternal motion; here is an atom with an evident endowment of eternal life. If it has that now, must its eternity not have existed in the past also?

Indeed so full of activity is matter, that even when atoms are in combination and are apparently at rest, in the centre even of the everlasting hills, they are for ever revolving or in agitation in their very limited sphere.

Roger Bacon—Is all this true? This system has brought about no true definition. So far as I can understand, most if not all the life is held to be in the definite atom; so far as many are concerned it certainly is. It has been supposed that there is one common atom as well as one substance; but it has been said that the atom possesses always more or less motion, due, it is assumed, "to a primordial impulse. This motion gives rise to volume; the more rapid the motion the greater the space occupied by the atom."

Let us look at the elements, and see if all is true. The metals are certainly not much inclined to combine amongst themselves. Mercury will attack some; but freeze it, and it is as quiet as the others, whilst soft metals are deadened by a similar treatment.

John Dalton—True the metals are cold-blooded, and require a hot climate to live in; but give them warmth, and you see another result. Even without warming them, put them to a cool liquid; platinum itself will give way before acids.

Roger Bacon—Yes, but freeze the acids, and I fear we come to the well-known saying, "Corpora non agunt nisi soluta."

John Dalton—But why trouble yourself with these frozen bodies? Take hydrogen and chlorine on the coldest day, and let them but get a glimpse of the sun, and they burst out with a joy unknown even to the violence of our youth. There we see the activity of elements.

Roger Bacon—True, but freeze your chlorine, not difficult to do; freeze your hydrogen into such a solid that it falls like a pebble on the floor. I fear they will have no joyful shout, but will not deign to recognise each other, even if the pieces are in apparent contact.

Nay more, take all the elements that you have in the world and freeze them to solids, throw them into a heap together as large as you please, and is it not almost certain from our not perfect knowledge that the action will be simply nothing. The elements are dead, the matter we see is dead, the matter concerning which men speak is dead. Here is an end; let that be left in peace.

John Dalton—That is easily granted if there is no warmth, but add only a small amount of heat to this mass of elements and will you not light up the whole? give even a little corner the light of a match and this will cause a combination, and this will produce heat, and this heat will warm up the next portion, and like a fire of the richest coal the whole will burn and spread heat to all around.

Here is the inherent power of the atom; it was not dead—it slept.

Roger Bacon—If so, what has become of the eternal motion of your atoms? what has become of the motion of your gases? what has become of the independent life of your matter? What power is in these masses to grow trees and thinking men? I thought you admired the theories of Lucretius, who tells us of the "primordia rerum," the very beginning of things moving gradually into complexity and evolving worlds and life. He was the earliest of the evolutionists whose system is well known to us, and he has many followers who somewhat modify his views. I thought you admired the self-initiating vortices by which are made all worlds, and the beautiful nebular theory of Laplace and of moderns now making worlds busily by this atomic eternal activity.

John Dalton—Not exactly. Moderns require heat, and heat itself is produced by atomic action, so that the action is enough after all.

Roger Bacon—Heat is motion. I thought it was agreed that the atoms would not move by themselves.

But as you require something more I will give it you. Indeed I will give you as much heat as you please near this corner, where you proposed to set the bulk on fire. Let there be a piece of any of our elements glowing white placed near. If heat is motion there must be something to move. By what means will this heat communicate motion to the elements?

John Dalton—Heat passes in a few minutes from us to the sun, and your small bundle of elements would soon absorb all you could send.

Roger Bacon—True it may be that heat passes to us rapidly, through space, but if it is only motion it must pass on something movable. Can you imagine it passing through empty space? Men used to think so when heat was to them a solid, but now there must be a something that is put into agitation. If your elements were quite alone they could not be fired; they must have that which we call ether, simply because it is so thin compared with our usual matter.

I will go farther. Even if your elements in combining produced heat, that heat would not be given out; to what place would it go? It could not go into empty space. What motion can there be in emptiness; can there be motion in nothingness? Your great fire would burn out, and the force would not be preserved. The waves would be lost for want of the sea; or it would be a dash against an immovable shore, from which too there was no rebound.

I may even go farther still. Let us suppose some of the elements heated: I doubt if this heat could be communicated to the others. I do not know that an atom can touch an atom; if not the heat could not be communicated through the empty space. This ether, unknown as it is, may be required to bridge over the same distance between atom and atom. Where, then, is the force of your atom and its power of life? To live it requires the aid of a something only beginning to be recognised as matter, and the ingenious Lucretius and all similar thinkers to this day have speculated on an empty supposition. We have seen that our matter is dead unless it has heat, that heat cannot be conveyed to it unless by means of a something that we must suppose to exist, but whose existence is unintelligible to us, and which has none of the properties usually assigned to matter. Moreover, even if this something is allowed to exist in abundance it cannot give heat, but can only convey it.

John Dalton—I am willing to concede the ether as matter and the heat as motion; then I think we have all the preparation for the heat which you require.

Roger Bacon—If you concede the ether as matter you go before many physicists and chemists. You observe the quotation I made from a very recent book that matter is that which possesses weight. I never heard of the ether gravitating. You concede then the point that the elements known to chemists do not show any activity or vitality of themselves, and they must be warmed before they act. You allow that the elements have not initiated chemical activity, and that is a great concession, and I think it only an honest one demanded by the known facts. You allow that we must bring in motion from the exterior. You allow that when we have it in any amount it will never reach the elements, however near, unless the ether intervene. You allow also, I think, that even the motion of heat will in all probability not pass from molecule to molecule, let us say, or our present chemical atoms. Surely then it is not far wrong to say of our elements, regarding them as the total matter of the universe; matter is dead.

III. A CONTRIBUTION TO THE HISTORY OF ELECTRIC LIGHTING.

By W. Mattieu Williams, F.C.S., F.R.A.S.

As the subject of lighting by electricity is occupying so much public attention, and the merits of various inventors and inventions are so keenly discussed, the following facts may have some historical interest in connection with it.

In October, 1845, I was consulted by some American gentlemen concerning the construction of a large voltaic battery for experimenting upon an invention, afterwards described and published in the specification of "King's Patent Electric Light" (Letters Patent granted for Scotland, November 26, 1845; enrolled March 25, 1846).

Mr. King was not the inventor, but he and Mr. Dorr supplied capital, and Mr. Snyder also held a share, which was afterwards transferred to myself. The inventor was Mr. Starr, a young man about 25 years of age, and one of the ablest experimental investigators with whom I have ever had the privilege of near acquaintance.

He had been working for some years on the subject, commencing with the ordinary arc between charcoal points. His first efforts were directed to maintaining constancy, and he showed me, in January of 1846, an arrangement by which he succeeded in effecting an automatic renewal of contact by means of an electro-magnet, the armature of which received the electric flow, when the arc was broken, and which thus magnetised brought the carbons together and then allowed them to be withdrawn to their required separation, when the flow returned. This device was almost identical with that subsequently re-invented and patented by Mr. Staite (quite independently I believe), and which, with modifications, has since been rather extensively used.

Although successful so far, he was not satisfied. He reasoned out the subject, and concluded that the electric spark between metals, the electric arc between the carbons, and other luminous electric phenomena are secondary efforts due to the heating and illumination of electric carriers; that the electric spark of the conductors of ordinary electrical machines is simply a transfer of incandescent particles of metal, which effect a kind of electric convection, known as the disruptive discharge; and that the more brilliant arc between the carbon points is simply due to the use of a substance which breaks up more readily, and gives a longer, broader, and more continuous stream of incandescent convection particles.

This is now readily accepted, but at that time was only dawning upon the understanding of electricians. I am satisfied that Mr. Starr worked out the principle quite originally. He therefore concluded that, the light being due to solid particles heated by electric disturbance, it would be more advantageous—as regards steadiness, economy, and simplicity—to place in the current a continuous solid barrier, which should present sufficient resistance to its passage to become bodily incandescent without disruption.

This was the essence of the invention specified in King's Patent as "a communication from abroad," which claims the use of continuous metallic and carbon conductors, intensely heated by the passage of a current of electricity, for the purposes of illumination.

The metal selected was platinum, which, as the specification states, "though not so infusible as iridium, has but little affinity for oxygen, and offers a great resistance to the passage of the current." The form of thin sheets known by the name of leaf-platinum is described as preferable. These to be rolled between sheets of copper in order to secure uniformity, and to be carefully cut in strips of equal width, and with a clean edge, in order that one part may not be fused before the other parts have obtained a sufficiently high temperature to produce a brilliant light. This strip to be suspended between forceps.

I need not describe the arrangement for regulating the distance between the forceps, for directing the current, &c., as we soon learned that this part of the invention was of no practical value, on account of the narrow margin between efficient incandescence and the fusion of the platinum. The experiments with the large battery that I made—consisting of 100 Daniell cells, with 2 square feet of working surface of each element in each cell, and the copper-plates about ¾ of an inch distant from the zinc—satisfied all concerned that neither platinum nor any available alloy of platinum and iridium could be relied upon; especially when the grand idea of subdividing the light by interposing several platinum strips in the same circuit, and working with a proportionally high power, was carried out.

This drove Mr. Starr to rely upon the second part of the specification, viz., that of using a small stick of carbon made incandescent in a Torricellian vacuum. He commenced with plumbago, and, after trying many other forms of carbon, found that which lines gas-retorts that have been long in use was the best.

The carbon stick of square section, about one-tenth of an inch thick and half an inch working length, was held vertically, by metallic forceps at each end, in a barometer tube, the upper part of which, containing the carbon, was enlarged to a sort of oblong bulb. A thick platinum wire from the upper forceps was sealed into the top of the tube and projected beyond; a similar wire passed downwards from the lower forceps, and dipped into the mercury of the tube, which was so long that when arranged as a barometer the enlarged end containing the carbon was vacuous.

Considerable difficulty was at first encountered in supporting this fragile stick. Metallic supports were not available, on account of their expansion; and, finally, little cylinders of porcelain were used, one on each side of the carbon stick, and about three-eighths of an inch distant.

By connecting the mercury cup with one terminal of the battery, and the upper platinum-wire with the other, a brilliant and perfectly steady light was produced, not so intense as the ordinary disruption arc between carbons, but equally if not more effective, on account of the magnitude of brilliant radiating surface.

Some curious phenomena accompanied this illumination of the carbon. The mercury column fell to about half its barometric height, and presently the glass opposite the carbon stick became slightly dimmed by the deposition of a thin film of sooty deposit.

At first the depression of the mercury was attributed to the formation of mercurial vapour, and is described accordingly in the specification; but further observation refuted this theory, for no return of the mercury took place when the tube was cooled. The depression was permanent. The formation of vaporous carbon was suggested by one of the capitalists; but neither Mr. Starr nor myself was satisfied with this, nor with any other surmise we were able to make during Mr. Starr's lifetime, nor up to the period of final abandonment of the enterprise.

When this occurred the remaining apparatus was assigned to me, and I retained possession of the finally arranged tube and carbon for many years, and have shown it in action worked by a small Grove's battery in the Town Hall of

Birmingham, and many times to my pupils at the Birmingham and Midland Institute.

These exhibitions suggested an explanation of the mysterious gaseous matter, which I believe to be the correct one, and also of the carbon deposit. It is this:—That the carbon contains occluded oxygen; that when the carbon is heated some of this oxygen combines with the carbon, forming carbonic oxide and carbonic acid, and a little smoke. I proved the presence of carbonic acid by the usual tests, but did not quantitatively determine its proportion of the total atmosphere.

If I were fitting up another tube on this principle I should wash it with a strong solution of caustic potash before filling with mercury, and allow some of the potash solution to float on the mercury surface, by filling the tube while the glass remained moistened with the solution. My object would be to get rid of the carbonic acid as soon as formed, as the observations I have made lead me to believe that—when the carbon stick is incandescent in an atmosphere of carbonic acid or carbonic oxide—a certain degree of dissociation and re-combination is continually occurring, which weakens and would ultimately break up the carbon stick, and increases the sooty deposit.

The large battery above described was arranged for intensity, but even then it was found that the quantity (I use the old-fashioned terms) of electricity was excessive, and that it worked more advantageously when the cells were but partially filled with acid and sulphate. A larger stick of carbon might have been used with the whole surface in full action.

After working the battery in various ways, and duly considering the merits of the other forms of battery then in use, Mr. Starr was driven to the conclusion that for the purposes of practical illumination the voltaic battery was a hopeless source of power, and that magneto-electric machinery driven by steam-power must be used. I fully concurred with him in this conclusion, so did Mr. King, Mr. Dorr, and all concerned.

Mr. Starr then set to work to devise a suitable dynamo-electric machine and, following his usual course of starting from first principles, concluded that all the armatures hitherto constructed were defective in one fundamental element of their arrangement. The thick copper-wire surrounding the soft iron core necessarily follows a spiral course, like that of a coarse screw-thread; but the electric current or lines of force which it is designed to pick up and carry circulate at right angles to the axis of the core, and extend to some distance beyond its surface. The problem thus presented is to wind around the soft iron a conductor that shall be broad enough to grasp a large proportion of this outspread force, and yet shall follow its course as nearly as possible by standing out at right angles to the axis of the armature. This he proposed to effect by using a core of square section, and winding round it a broad ribbon of sheet copper, insulated on both sides by cementing on its surfaces a layer of silk ribbon. This armature to be laid with one edge against one side of the core, and carried on thus to the angle; then turned over so that its opposite edge should be presented to the next side of the core; this side to be followed in like manner, the ribbon similarly turned again at the next corner, and so on till the core becomes fully enclosed or armed with the continuous ribbon, which would thus encircle the core with its edges outwards, and nearly at right angles to the axis, in spite of its width, which might be increased to any extent found by experiment to be desirable.

At this stage my direct co-operation and confidential communication with Mr. Starr ceased, as I remained in London while he went to Birmingham in order to get his machinery constructed, and to apply it at the works of Messrs. Elkington, who had then recently introduced the principle of dynamo-electric motive-power, electro-plating, &c., and were, I believe, using Woolrich's apparatus, the patent for which was dated August 1, 1842, and enrolled February 1, 1843.

I am unable to state the results of his efforts in Birmingham. I only heard the murmurs of the capitalists, who loudly complained of expenditure without results. They had dreamed the same dream that Mr. Edison has recently re-dreamed, and has told the world so loudly. They supposed that the mechanically excited current might be carried along great lengths of wire, and the carbons interposed where required, and that the same electricity would flow on and do the duty of illumination over and over again, as a river may fall over a succession of weirs and turn water-wheels at each. Mr. Starr knew better; his scepticism was misinterpreted; he was taunted with failure and non-fulfilment of the anticipations he had raised, and with the fruitless expenditure of large sums of other people's money. He was a high-minded, honourable, and very sensitive man, suffering already from overworked brain before he went to Birmingham. There he worked again still harder, with further vexation and disappointment, until one morning he was found dead in his bed. Having, during my short acquaintance with him, enjoyed his full confidence in reference to all his investigations, both completed and incomplete, I have no hesitation in affirming that his early death cut short the career of one who otherwise would have largely contributed to the progress of experimental science, and have done honour to his country. His martyrdom, for such it was, taught me an useful lesson I then much needed, viz., to abstain from entering upon a costly series of physical investigations without being well assured of the means of completing them, and, above all, of being able to afford to fail.

There are many others who sorely need to be impressed with the same lesson, especially at this moment and in connection with this subject.

The warning is the most applicable to those who are now misled by a plausible but false analogy. They look at the progress made in other things, the mighty achievements of modern Science, and therefore infer that the electric light—even though unsuccessful hitherto—may be improved up to practical success, as other things have been. A great fallacy is hidden here. As a matter of fact the progress made in electric lighting since Mr. Starr's death, thirty-one years ago, has been very small indeed. As regards the lamp itself no progress whatever has been made. I am satisfied that Starr's continuous carbon stick, properly managed in a true vacuum, or an atmosphere free from oxygen, carbonic oxide, carbonic acid, or other oxygen compound, is the best that has yet been placed before the public for all purposes where exceptionally intense illumination (as in lighthouses) is not demanded. It is the steadiest, the cheapest, and least glaring in proportion to the amount of light it radiates. It has not been "pushed" like other devices, simply because it is nobody's exclusive property.

Comparing electric with gas lighting, the hopeful believers in progressive improvement appear to forget that gas making and gas lighting are as susceptible of further improvement as electric lighting, and that, as a matter of fact, its practical progress during the last forty years is incomparably greater than that of the electric light. I refer more particularly to the practical and crucial question of economy. The by-products, the ammoniacal salts, the liquid hydrocarbons, and their derivatives, have been developed into so many useful forms by the achievements of modern chemistry, that these, with the coke, are of sufficient value to cover the whole cost of manufacture, and leave the gas itself as a volatile residuum that costs nothing. It would actually and practically cost nothing, and might be profitably delivered to the burners of gas consumers (of far better quality than now supplied in London) at one shilling per thousand cubic feet, if gas making were conducted on sound commercial principles,—that is, if it were not a corporate monopoly, and were subject to the wholesome stimulating influence of free competition and private enterprise. As it is, our gas and the price we pay for it are absurdities; and all calculations respecting the comparative cost of new methods of illumination should be based not on what we *do* pay per candle-power of gas-light, but what we *ought* to pay and *should* pay if the gas companies were subjected to desirable competition, or visited with the national confiscation I consider they deserve.

Having had considerable practical experience in the commercial distillation of coal for the sake of its liquid and solid hydrocarbons, I speak thus plainly and with full confidence.

There is yet another consideration, and one of vital importance, to be taken into account, viz., that—whether we use the electric light derived from a dynamo-electric source, or coal-gas—our primary source of illuminating power is coal, or rather the chemical energy derivable from the combination of its hydrogen and carbon with oxygen. Now this chemical energy is a limited quantity, and the progress of Science can no more increase this quantity than it can make a ton of coal weigh 21 cwts. by increasing the quantity of its gravitating energy.

The demonstrable limit of scientific possibilities is the economical application of this limited store of energy, by converting it into the demanded form of force without waste. The more indirect and roundabout the method of application, the greater must be the loss of power in the course of its transfer and conversion. In heating the boiler that sets the dynamo-electric machine to work, about one-half the energy of the coal is wasted, even with the best constructed furnaces. This merely as regards the quantity of water evaporated. In converting the heat force into mechanical power—raising the piston, &c., of the steam-engine—this working half is again seriously reduced. In further converting this residuum of mechanical power into electrical energy, a further and considerable loss is suffered in originating and sustaining the motion of the dynamo-electric machine, in the dissipation of the electric energy that the armature cannot pick up, and in overcoming the electrical resistances to its transfer.

I am unable to state the amount of this loss in trustworthy figures, but should be very much surprised to learn that, with the best arrangements now known, more than one-tenth of the original energy of the coal is made practically available. This small illuminating residuum may, and doubtless will, be increased by the progress of practical improvement; but, from the necessary nature of the problem, the power available for illumination at the end of the series must always be but a small portion of that employed at the beginning.

In burning the gas derived from coal we obtain its illuminating power *directly*, and if we burn it properly we obtain nearly all. The coke residuum is also directly used as a source of heat. The chief waste of the original energy in the gas-works is represented by that portion of the coke that is burned under the retorts, and in obtaining the relatively small amount of steam-power demanded in the works. These are far more than paid for by the value of the liquid hydrocarbons and the ammonia salts, when they are properly utilised.

In concluding my narrative I may add that after Mr. Starr's death the patentees offered to engage me on certain terms to carry on his work. I declined this, simply because I had seen enough to convince me of the impossibility of any success at all corresponding to their anticipations. During the intervening thirty years I have abstained from further meddling with the electric light, because all that I had seen then, and have heard of since, has convinced me that—although as a scientific achievement the electric light is a splendid success—its practical application to all purposes where cost is a matter of serious consideration is a complete and hopeless failure, and must of necessity continue to be so.

Whoever can afford to pay some shillings per hour for a single splendid light of solar completeness can have it without difficulty, but not so where the cost in pence per hour per burner have to be counted.

I should add that before the publication of King's specification, Mr. (now Sir William) Grove proposed the use of a helix or coil of platinum, made incandescent by electricity, as a light to be used for certain purposes. This was shown at the Royal Society on or about December 1st, 1845.

IV. THE PROBLEM OF FLIGHT: BALLOONING IN ARCTIC EXPLORATION.

THE proposal to employ balloons in Arctic Exploration has recently attracted more general attention to the hitherto unsolved problem of artificial flight. The methodical study of the laws of natural flight, which has been pursued for several years by Dr. Pettigrew, Prof. Marey, and M. Dupuy de Lome, has cleared up many disputed points. The researches of these gentlemen have already been fully described and illustrated in the "Journal of Science." Our object in again directing the attention of our readers to the subject is not therefore to refer to their labours, but to give a brief account of the more recent experiments of Mr. Brearey, the indefatigable Secretary of the London Aëronautical Society. Mr. Brearey has been studying the subject for some years, and has communicated to the Society the results he has obtained, illustrating his paper by models the bird-like action of which becomes, as Mr. Glaisher remarked, very interesting when we consider that it is produced by mechanism. By following up these experiments our knowledge of the subject must of necessity be largely increased, if the problem of flight be not actually solved.

Mr. Brearey first illustrated flight by means of models projected by the hand. The first experiment demonstrated flight by gravity alone, showing how the bat, hanging by its claws, by simply releasing itself attains its first flight.

Then he showed how the application of force neutralises the force of gravity. In this model the screw propelled a plane surface, which was represented by wings. It was made after the model of M. Penaud, of the French Aëronautical Society, improved as to the screw by Mr. Brearey.

To show that different forms of surface may be employed with instructive results for future work, the author in the next model adapted the albatross form of wing, the model being about half the length, viz., 7 feet, but the breadth being only one-fourth, or 2 inches, that of the albatross being about 8 inches.

This class of experiment may be greatly varied with a view to ascertain the weight which can be carried under a given surface. According to Mr. Brearey the angle of inclination with which the wing advances will have to be increased with the weight, and also the force in the same relative proportion.

It is asserted by some naturalists, in explanation of effective wing action, that the feathers of a bird's wing are made to underlap each other, so that in the downward stroke the pressure of the air closes them upwards against each other, and converts the whole series into one connected membrane, through which there is no escape; whilst in the upward stroke the same pressure has precisely the reverse effect. "It opens the feathers," says the Duke of Argyll, "separates them from each other, and converts each pair of feathers into a self-acting valve through which the air rushes at every point." The Duke, in his "Reign of Law," remarks Mr. Brearey, so thoroughly recognises, in another place, the immense importance of the concave and convex surfaces in gripping the air in the one case and evading it in the other, that he can scarcely lay much stress upon the valvular system of feathers. Dr. Pettigrew, whose researches give weight to his statement, estimates this difference as two to one.

To show that in the wing propeller the convex and concave arrangement is most effective, Mr. Brearey stood upon a pivoted stool, and holding the artificial wing perfectly level, waved it up and down, by which action he was caused to revolve.

He then proceeded to the practical application of the concave-convex theory by exhibiting a model after the construction of M. Penaud, of the French Society.

Experimenting with various forms of wings, he has been enabled to imitate the leisurely flight of the crow and the swift flight of the swallow. Mr. Brearey also made some observations upon the vertical screw, again resorting to M. Penaud's Helicoptère in illustration.

Passing to the question of the use of balloons in Polar Exploration, the author remarked that the balloon has a sphere of its own quite independent of its shape, unapproachable by any other invention, and the question for discussion is—Was the late Polar Expedition such an opportunity as afforded any reasonable chance for the useful employment of the balloon?

This question can never be fairly answered unless a balloon, with the necessary means for its inflation, form part of the vessel's equipment.

All the materials and apparatus being conveyed to the place of destination, there should be no difficulty in the inflation. Giffard's balloon, exhibited in 1869 at Cremorne, was inflated with pure hydrogen, and could carry upwards of 16 tons.

The balloon successfully inflated, then what would be the work expected from it?

Instead of a seventy days' journey, to accomplish about 70 miles, at a fearful cost of life and suffering consequent upon having to drag over ice hummocks sledges containing provisions, as described in the Arctic Report, Mr. Brearey argues that the whole of the stores could have been conveyed over the heads of the explorers, and the men holding the ropes of this floating observatory would have been assisted by the upward tendency of the balloon. The question is—Would the daily consumption of stores compensate the leakage of gas? Major Beaumont, in his history of the Balloon as employed in the American War, says "that the balloon when inflated can, unless in very windy weather, be very readily carried. Twenty-five or thirty men lay hold of cords attached to the ring and march along, allowing the machine to rise only sufficiently to clear any obstacle. He had frequently," he says, "seen it carried thus without the least difficulty." He further says "that there was always a small amount of leakage, but, from the superiority of the varnish, at the end of a fortnight, sufficient gas remained in the balloon to enable an ascent to be made without its being replenished." The ascensive power of a balloon thus conveyed for purposes of war must be available at any moment for the two observers, and the additional weight of the two guy ropes which it also has to sustain, so that the necessity for the twenty-five or thirty men is explained; but for the purposes of exploration and the carrying of stores a very few pounds of ascensional force need be requisite. These stores, however, upon being removed from the balloon; or the sledges, which might be partly buoyed by the balloon, being detached; could not the balloon then be utilised to survey the country from some thousand feet or more by means of a let-out cord?

An interesting discussion followed the reading of Mr. Brearey's paper, in the course of which Mr. Reece remarked that there would be no fear of the efficacy of hydrogen. After it was generated it would pass through ice, or would be so cold that it would maintain the same temperature throughout the journey. Hydrogen gas would be generated at a heat of 180°. It would then pass through a tube and be chilled, and would remain at a temperature of about 32°, so that there would be no fear of its depositing a mass of snow or ice. Mansfield, in his work on ballooning, suggested that the weight of a man might be taken off by ballooning. A balloon of 18 feet diameter would take off the weight of a man; and in this way a man named Ward was able to leap in the forest, from tree to tree, with a velocity of 15 miles an hour. In this case a man might guide a sledge of dogs at a great pace, and could convey stores by this means to any point.

With regard to the nature of hydrogen gas in a severe frost, Mr. Reece said he had submitted the gas to intense cold, and it appeared to have no effect upon it. Faraday exposed it to cold 100° below zero and a pressure of 800 atmospheres, and never found that either had the slightest effect upon it; neither had the most intense cold or pressure that he could produce at the Royal Institution.

Mr. Simmons remarked that the hot-air balloon seemed to be the best adapted to the especial object—

1. Because in the presence of intense cold, wind does not exist, wind being the chief drawback to the inflation of hot-air balloons in England. (Mr. Simmons's experience in this matter was confined to Canada; he had not visited the Arctic Regions.)
2. Because the more intense the cold of the air surrounding the balloon, the greater the ascending power.
3. The hot-air balloon during inflation will give off sufficient heat from its surface to keep the men warm whilst they are holding the net, and when the balloon is afloat no inconvenience can be experienced from cold.
4. The time required for the inflation of the hot-air balloon is about half-an-hour, and the preparation of the apparatus for the inflation will never be found so troublesome or occupy so much time as that for the hydrogen balloon.
5. The danger and annoyance from carrying oil of vitriol will be obviated.
6. Hot-air balloons have no preparation spread upon their surfaces that can sustain any injury, decomposition, or spontaneous combustion from being closely packed for a lengthened period.

The entire weight of the balloon apparatus used at the Royal Arsenal, Woolwich, was 1200 lbs., its diameter was 70 feet, and the heat when inflated, taken 10 feet above the open neck of the balloon, was 120° F.

The greatest difficulty against the inflation of a balloon with pure hydrogen gas in intensely cold regions would, in

Mr. Simmons's opinion, be the keeping of the water in the retort from freezing whilst charging or after being charged with water, until the vitriol is poured in. The process of making pure hydrogen gas by means of furnaces would necessitate the employment of exceedingly cumbersome apparatus.

Mr. Reece, alluding to the formation of hydrogen, remarked that, according to Sir George Nares, the average temperature during the Expedition in the Arctic Regions was 30° F. That would not have the slightest effect on a composition of 1 part sulphuric acid and 4 parts water. When poured on zinc the temperature would rise to 180°. If anyone tried that in a glass vessel he could not keep his hand on it, so that any fear of not generating the gas must be entirely visionary. Air expands only one 480th part for each degree, so that great heat would be required for an air balloon. A fire balloon had enormous power, but nothing like one filled with hydrogen gas.

Mr. Simmons remarked that the heat generated in a hot-air balloon would be 120°. The weight of a balloon and all its paraphernalia might be 1200 lbs., the diameter 70 feet, and it would carry a man into the air if the average heat were 120°.

Mr. Glaisher, in thanking Mr. Brearey for his paper, observed that in the late Arctic Expedition everything was excluded that was possible to be excluded on account of the want of room. If a balloon of 70 feet diameter had to be taken out, a very large space would be required. Again, it could only be used in summer time, when there is wind in the Arctic Regions. In Russia and Sweden in winter time, when the temperature approaches zero, it is nearly always calm. To realise the intensity of the cold one must move the hand against the cold air or run against the air. No person standing in an atmosphere 70° below zero would feel that the cold was so intense. It might be far more painful when the temperature was above zero if the air were in motion; but the winter was not the time when these experiments would be made: they would take place in the summer, when the temperature would be 40°, and in the sun very much hotter. He (Mr. Glaisher) saw no reason, however, why the balloon should not be made available in various ways in Arctic Exploration, and he hoped that if there was another Expedition the balloon would be tried and the question settled. It would certainly, if used in connection with a sledge, enable the distance that could be traversed in the day to be increased. With regard to the view that could be obtained from the balloon: when he was half a mile over London he could see Margate and Brighton, and on to the Norfolk coast. This showed how much may be seen from a comparatively small elevation. From the height of a mile he could see nearly 90 miles, and even when a few hundred feet high one was in a position to see over the country for several miles ahead. In any case he hoped that in the next Expedition, from whatever country it may proceed, not only one balloon but several balloons would be taken out.

V. ELECTRIC LIGHTING BY INCANDESCENCE.

To determine the Electromotive Force required to maintain a rod of a given material, of given dimensions, at a given temperature, when loss of heat by radiation, convection, and conduction are taken into account.

By Prof. W. E. Ayrton.

HAVING found it necessary to solve the above question in connection with the possibility of economically illuminating, by a material rendered incandescent with the electric arc (the system employed by Edison, Wedermann, Sawyer-Man, &c., and *first* patented in England by De Moleyns in 1841, and not, as commonly supposed, by King, since his patent bears the date of 1845), I have thought that the results obtained may be generally interesting.

The problem of the flow of heat through a bar from which there is loss of heat by radiation and convection has, since the time of Fourier, been often treated; the mathematical investigation was employed by Forbes in his experiments made to determine whether the heat conductivity of iron, like the electric conductivity, varied with the temperature, and exactly similar equations have been made use of in studying the passage of an electric current through a wire from all points of which there is leakage, as in an ordinary submarine cable, and especially in a submarine cable made of inferior gutta-percha. The problem, however, we have on the present occasion to deal with differs from the preceding in that there is, at every point, not only loss of heat by conduction, radiation, and convection, but, in addition, a generation of heat due to the resistance opposed to the passage of the electric current.

The method of solution that must be employed consists in first calculating the amount of heat that will be lost per second by conduction, convection, and radiation in each very small section of the bar, as well as the amount generated per second by the electric current in the same section. Then, when the temperature of the bar has settled down into a constant state, that is when the temperature of any one point does not vary from second to second (although the temperature of different points of the bar may be very different at the same time), the total loss and the total generation of heat per second in any one section must be equal. This equality leads to a differential equation, the solution of which is an exponential formula, connecting the strength of the current required with the temperature of the middle point of the wire, its dimensions, specific resistance, heat conductivity, and surface emissivity.

In order to apply this formula to the two very important cases of illuminating by an incandescent platinum wire, or by an incandescent carbon rod, it is necessary to know the surface emissivity, or heat radiating power, of the substance of our incandescent wire.

Now probably no experiments have yet been made to determine the *absolute* loss of heat per unit of surface from a body of a given temperature when the difference of temperature between the heated surface and the walls of the room exceeds 100 or 200 degrees centigrade. And, in fact, when this difference is even less than 100 degrees the *accurate* information at our disposal is scanty. Fortunately, however, the law of radiation of heat is such that unless there is a total discontinuity in the phenomenon as the temperature rises we are enabled to make a very good rough estimate of what this absolute amount of radiation must be, even at very high temperatures, such as that of white-hot charcoal.

Formerly, it was thought that Newton's law of cooling, viz.:—that the rate of loss was simply proportional to the excess of temperature, was correct; next the experiments of MM. Dulong and Petit seemed to show that in a vacuum the rate of loss was proportional to $m \; 1{\cdot}0077_{\theta} \; (1{\cdot}0077^{t} — 1)$ where m is a constant depending on the radiating surface, θ the temperature of the enclosure, and t the excess of temperature of the hot body; while those of Mr. Hopkins led to the result that the loss by the convective action *alone* of the gas in which the heated body was placed was proportional to $m\,p_a\,t^{1\cdot233}$ where m and a depend on the nature of the gas at a pressure p, and t, as before, is the excess of temperature.

The preceding formulæ do not, however, give results agreeing well with more recent experiments. In the "Proceedings of the Royal Society" for 1872, Mr. McFarlane published the results of his experiments on the absolute amount of radiation and convection from a polished and from a blackened copper ball in air. In addition to the fact that these experiments were made probably under the eye of Sir William Thomson, to whom Mr. McFarlane is an assistant, and that, therefore, they are probably correct, I have verified them experimentally myself, and extended them to other gases than air, and at various pressures.[*]

Mr. McFarlane's experiments show that the measured surface emissivity increases *less* and *less* rapidly as the difference of temperature rises. Probably, therefore, we may be safe in saying that for a difference of temperature of 1000° C. (about the temperature of melting cast-iron), the value for platinum and carbon will not be very much higher than the highest he obtained, respectively, for polished and for blackened copper.

From experiments made in a vacuum, I find that when the difference of temperature is under 100° C. the value of the surface emissivity is about half, and for gases like coal-gas about double that obtained by Mr. McFarlane in damp air at ordinary pressure.

In some of the systems of electric lighting it has been proposed to enclose the incandescent rod in a vacuum, in others in some non-decomposable gas, like nitrogen. In this latter gas I have not hitherto made absolute radiation and convection experiments, but we may fairly take as a mean value of this surface emissivity the air value previously referred to.

The specific electric resistance of platinum is a comparatively constant quantity, while that for carbon varies within wide limits. It, therefore, becomes an important question to determine what kind of carbon, that which conducts electricity pretty well, or that which is a much worse conductor, is the more suitable to be used for illumination by incandescence. Now the temperature equation previously referred to, shows that for a given electromotive force maintained at the two ends of a carbon rod, the smaller be the specific resistance of the carbon employed, the greater will be the heat, and, consequently, temperature produced.

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[*] The results of these experiments of Professor Perry and myself will shortly be published.

Of course, this is on the assumption that the heat conductivity does not vary with the electric conductivity, and this seems to a certain extent to be the case, since, while we find the electric conductivity of carbon varying between very wide limits, the heat conductivity, as far as results of experiments are available, seems to be much more constant.

At first sight it might appear inconsistent with ordinary experience to say that for a carbon rod of given dimensions the smaller the specific resistance the greater the quantity of heat developed, but it must be remembered that here we are considering a *constant electromotive force* applied at the two ends of the rod, whereas in ordinary practice it is a *constant current* we have to deal with, since when even a Grove's battery is employed to produce the current through a *short continuous* rod of carbon, the chief resistance is in the battery itself, so that the current is practically independent of changes of resistance in the carbon.

Consequently, for this investigation, it is better to employ a form of carbon opposing a low resistance, than one offering a high resistance to the passage of the current.

Substituting in our equation the known values of the heat conductivity, &c., of platinum and carbon, we arrive at the result that if our incandescent wire be about No. 20, Birmingham wire gauge, or have a diameter of about one millimetre, and if its length be five centimetres in the case of the platinum and two in the case of the carbon (this longer length in the first instance being necessary on account of the better conducting power of the platinum), then the electromotive force necessary to be maintained at the two ends of the wire is—

0·2848 volts, or roughly one-third of that of a Daniell's cell, in the case of the platinum, and

0·46013 volts, or roughly one-half that of a Daniell's cell, in the case of the carbon wire.

It is, therefore, quite possible to produce a light with an electromotive force far less than that of a single Daniell's cell, but not, of course, with a Daniell's cell itself, since the resistance of this would be incomparably greater than that of our incandescent rod or wire.

Although it requires 50 per cent more electromotive force to maintain the centre of our two-centimetre rod of carbon at 1000° C. than it does to maintain the centre of our five-centimetre length of equally thick platinum wire at the same temperature, it is probable that the shorter carbon rod would give out more light—first, because, in consequence of the inferior heat conductivity of carbon, the temperature curve would be flatter in the middle and steeper at the ends, so that the "surface integral" of the temperature would be higher in the case of the carbon than for the platinum; secondly, because a substance like carbon, which is black when illuminated by an extraneous light, gives out more light than a bright substance like platinum, at the same temperature, when sufficiently heated to become self-luminous; a phenomenon easily tried by placing in the flame of a spirit lamp a piece of platinum foil on which a black spot has been made with Indian ink.

In the preceding investigation no reference has been made to the value of the specific heat of the substance rendered incandescent; and reflection will show that, if a continuous light be required, the specific heat can (contrary to what has been often said) have nothing to do with the question; for, if the electric current only produces per second as much heat as will be lost per second by radiation, convection, and conduction after the body has reached a temperature of 1000° C., then it must necessarily reach this temperature, since at all lower temperatures it will gain more heat than it loses.

If, however, instead of a constant light, it were, for any purpose, desired that a rapid succession of lightings and extinguishing of the lamp should be possible, then, undoubtedly, it would be well to employ, for the incandescent rod, a substance having a small specific heat.

I found it not difficult, in 1873, when designing, at the request of the Government, an electric lamp for the use of the divers who recovered the property sunk in the French Mail steamer *Nil* off the coast of Japan, to make such thin carbon wires as I have spoken of in this paper. The process I employed was first to scrape an ordinary carbon rod very thin, then fix it to the two electrodes inside a glass globe from which the air could be exhausted. A strong electric current was now passed through the rod, which was burnt to the required degree of thinness by the air pumped through the globe. The current was then stopped, the air pump out, and replaced by nitrogen, when the globe was sealed up.

Another plan I employed, on account of the fragile nature of very thin carbon, was to use platinum wire coated with carbon, and rendered incandescent in an atmosphere of nitrogen, but it was difficult to prevent the carbon coming off the smooth platinum wire.

VI. INSTINCT OR REASON?*

HERE we wish to place on record a narrative of a most singular occurrence witnessed by the writer and a friend while hunting ducks at Bean's Lake, in Piatte County, Missouri. It will be found a most vivid illustration of the wonderful intelligence of a bird whose habits of solitary life have kept it beyond the ordinary observation of men.

While sitting behind a "blind," over our decoys, waiting for game, on the 3rd day of April, 1875, we saw, at the distance of a mile toward the west end of the lake, a large number of huge white birds, and, watching them curiously, finally discovered that they were coming toward us on the water—a host. In an hour the mass was abreast of us, and proved to be made up of pelicans, a band we estimated to be 60 feet in width and 300 feet long closely packed together, each one touching his neighbours on all sides. It appeared as though a foot-ball could not have fallen to the water at any point on the band. They moved as a raft might move with a slow current—no motive power visible, each one maintaining his place without flutter or struggle. Moreover, they were unlike swan or geese in that their heads were not raised, being drawn backward and laid upon the back between the wings, while the long mandibles lay forward, flat upon the neck. Not a sound or a motion from an individual; only the still, solemn glide of the entire raft. And yet there were many thousands of them. Allowing a foot in width and a foot and half in length for each bird, there were twelve thousand pelicans before us. They passed on to the lower end of the lake, then almost immediately rose and flew back over us, and settled again in the shoal water. While flying the pelican shows the broad black band on the extreme feathers of the wings, and would be a very beautiful bird, but it does not stretch forth the neck, even while flying, thus appearing like a swan with his head cut off—*very awkward*. During the day this vast multitude was joined by scores of large flocks, which came down from the sky, circling down in narrow spirals from a height from which they could not be seen. Whence did they come? What was the object of the meeting? Who was the messenger that had called this horde together? They must have gathered from all the corners of the earth; for by extensive inquiry made since that day among those most likely to know, no witness has been found who ever saw more than fifty together, in one place in this region. All day long they moved slowly about the upper end of the lake, and only occasionally did small groups fly about or splash in the water. A great and solemn convention was evidently being held. Toward evening, however, we saw more commotion among them, and, desiring to witness more closely this wonderful gathering, we went up the lake toward them. After quietly approaching within a half mile of the nearest, we saw that a general motion was being made in our direction, whereupon we concealed ourselves close to the edge of the water, and from thence during the next hour witnessed the most amazing pageant of our lives.

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* From an article by Ermine Case, Jun., in the Kansas City Review.

The number had clearly doubled since morning, and the entire troop were arranged in line and engaged in a movement the object of which we could not at first make out. The line was some 300 yards in extent, and at least thirty birds in depth—all "dressed" to the left (continuing the military figure), and at that wing resting upon the bank from which the line slightly diverged, at the extreme right being some 30 yards out; the general movement being toward the shore. As the birds on the left reached the land, they immediately rose in the air and passed back of the line, and formed again on the extreme right. In this way probably five hundred birds were flying at a time, and by this method of deploying from the left to the right wing the grand army was being rapidly moved toward us. When quite near and before us we easily saw that the object of this advance in order of battle was not mere dress parade—each bird was rapidly dashing his head under water, bringing up fish. This long line, then, was a seine fastened to the bank at the left, and being gradually drawn toward the shore, driving the finny prey into the shallow water, where it was incontinently swallowed by the living meshes of this enormous net. Nothing could be of more thrilling interest than to witness the intelligence with which this enormous flock carried out every detail to success. Several times we noticed a detachment, as if detailed for special duty, swing around over some deeper pool, flying so as to drag their feet and flap the extremities of their wings upon the water, closing in finally at the open end of the net on the right. Had a skiff load of boys been sent out with poles to splash the water and drive in the fish, the manœuvre could not have been plainer in its object than was the one before us.

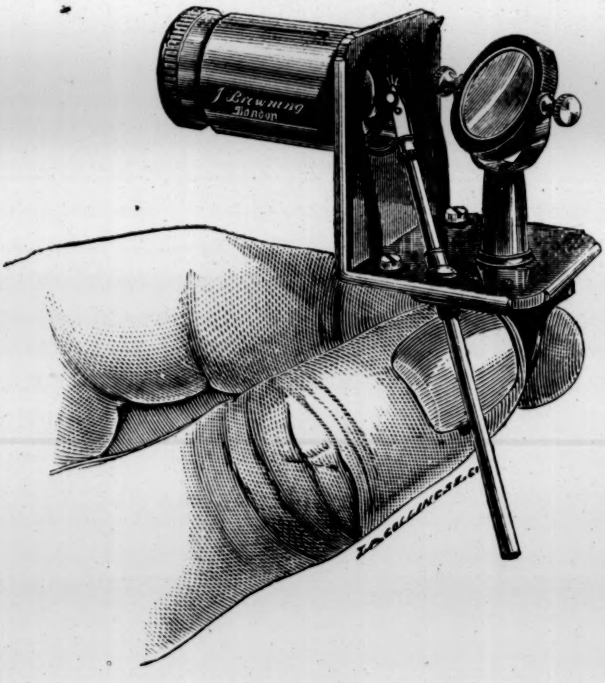
Why should one flying group "drive" in this way, while other scores passed regularly on to their places? Where was the commander-in-chief? There was no struggle for precedence, no fighting, no pressing out of line; here was a voiceless, disciplined multitude, intelligently carrying out an elaborate plan.

When the line had advanced, and lay within 30 feet of us, we rose quickly and fired our guns over them; and when this myriad rose, utterly filling the air, the broad wings crashed against one another with the roar of a hundred trains. Thus closed this most wonderful spectacle.

A specimen we obtained measured 9½ feet from tip to tip of the wings. The mandibles were 15 inches long, and the pouch held 2 quarts of wheat.

VII. COMPOUND ACHROMATIC MICROSCOPE.

MR. JOHN BROWNING has constructed a compound achromatic microscope of extremely small dimensions, the actual size being represented in the accompanying woodcut. The two powers magnify respectively about 15 and 35 diameters: the highest power is provided with a lieberkuhn for the illumination of opaque



objects, which may be held in the forceps, or, if suitably mounted with black spot, examined on the ordinary 3 x 1 slide, for holding which a spring clip is provided. A little practice with the mirror will enable great variety in illumination to be obtained. The definition is remarkably good, showing the markings of some of the coarser diatoms, such as Arachnoidiscus and Isthmia, and will prove useful as a demonstration microscope, as it can easily be passed from hand to hand with little chance of the adjustments becoming deranged. It will be also very useful to those who, from unsteadiness of hand, are unable to use a lens of tolerably high power, as no amount of tremor can interfere with its definition, and when packed in its case it can easily be carried in the waistcoat-pocket.

Mr. Zentmayer has contrived a stage for his new microscope which has virtually *no thickness whatever*. By placing the slide below the stage instead of above, as usual, every impediment to employment of illuminating pencils of extreme obliquity is done away with. The object is secured beneath the stage by a pair of spring clips. The whole of this very simple contrivance can be removed at pleasure, and the usual stage substituted when required.

VIII. SAFE ANÆSTHESIA.

THE dangers attending the administration of chloroform as an anæsthetic agent have always been a source of great anxiety to surgical operators; but while many other so-called safe anæsthetic agents have from time to time been proposed, it has hitherto been impossible to omit chloroform from its position as the only practical anæsthetic in surgical operations of a protracted nature. It will, however, be a source of great gratification to the whole of the members of the medical profession to know that the subject is under investigation, and that a competent committee are endeavouring not only to discover wherein the special dangers of chloroform consist, but also to discover if some anæsthetic agent can be found which will avoid these dangers. The Committee of the British Medical Association, consisting of Dr. Coats, Pathologist to the Western Infirmary, Glasgow; Dr. Ramsay, Assistant to the Professor of Chemistry; and

Dr. McKendrick, Professor of Physiology in the University of Glasgow, have already issued two reports, abstracts of which we propose to lay before our readers:—

In studying the question of the effects of chloroform on the respiration and the heart, it soon became apparent to the committee that chloroform administered to dogs and rabbits has a disastrous effect on the respiratory centres; it is easy to kill one of these animals by pushing the chloroform till respiration is paralysed. In observing the state of the heart during these experiments, it could often be determined by auscultation that its contractions were maintained after respiration had ceased. It was apparent, however, that, even when failure of respiration was more directly the cause of death, the heart was to some extent simultaneously affected; and there were even cases in which the heart appeared to fail at least as soon as, if not before, the breathing. Considering these facts, and bearing in mind that failure of the heart is often asserted in the reports of death from chloroform, a method of experimentation was devised by which respiration would be eliminated, and the effects of chloroform on the heart observed apart from that complication.

In the frog the movements of respiration are not necessary to life, so far as the heart is concerned, as that organ continues beating long after these movements have ceased. After exposing this animal to the vapour of chloroform under an inverted jar until it was anæsthetised, it was found that the heart became rapidly weaker, till it ceased beating, but with ether the heart continued vigorously beating as long as the experiment was continued.

A method was now devised for warm-blooded animals, such as rabbits and dogs. It was found that chloroform immediately produces a serious effect on the heart; the right ventricle almost immediately begins to distend, and the heart presently stops with the right ventricle engorged with blood. In the case of rabbits the heart often virtually came to a standstill within a minute of the introduction of chloroform. Ether may, however, be given under the same conditions for an indefinite period without interfering with the heart. Artificial respiration with ether was maintained in the circuit for an hour, and at the end of that time the heart was beating as vigorously as at first.

In this respect, therefore, ether possesses an enormous advantage over chloroform, but ether has the great disadvantage of tardiness of action. In comparative experiments with rabbits, in which the anæsthetics were given on a towel, it appeared that with chloroform complete anæsthesia was produced in about three minutes, while in the case of ether it took fifteen to twenty minutes, although the cloth was kept saturated. The committee therefore endeavoured to find an agent which should be as potent as an anæsthetic as chloroform, yet affect the heart and respiration as little as ether.

Whatever substitute for chloroform is found it must be one which can be administered in large doses, and in trying the following agents the committee administered full doses.

Benzine (C_6H_6) was used with the frog. Its effects were nearly as slow as those of ether, and it produced struggling; weakening of the heart was apparent, but not so great as with chloroform.

Acetone (C_3H_6O) produced only slight anæsthesia in the frog, even after prolonged administration.

Pyrrol (C_4H_5N) produced anæsthesia in the frog with considerably less rapidity than chloroform, but great excitement and muscular spasms took place before complete anæsthesia. Administered to three young rabbits subcutaneously, it produced convulsive movements, chiefly of the jaws and fore paws. Anæsthesia in these rabbits was doubtful.

So-called bichloride of methylene, the reputed formula of which is CH_2Cl_2, on being administered to frogs, it was found that the heart became quickly affected, and soon stopped. With rabbits, respiration rapidly deteriorated and stopped while the heart was still beating. In an experiment with artificial respiration the heart was weakened and soon stopped, but not as rapidly as with chloroform. As in the case of chloroform, the right ventricle became enormously distended—the first sign of paralysis being the commencement of this distension.

Amylene (C_5H_{10}) was administered to rabbits both by cloth and subcutaneously. No anæsthetic effect was produced.

Butyl chloride (C_4H_9Cl) administered to rabbits affected respiration, but not very rapidly. In experiments with exposure of the heart, the cardiac pulsations became weaker, and ceased altogether after some time. In one experiment, it was noted that, almost immediately after complete anæsthesia, the respiration became shallow and soon stopped.

Ethene dichloride, formerly named ethylene dichloride, or Dutch liquid ($C_2H_4Cl_2$) produced convulsive movements of both extremities, continuing up to death. There was no anæsthesia up to the commencement of the convulsions.

Methyl chloride (CH_3Cl), which boils at the ordinary temperature, was obtained in alcoholic solution in a sealed tube, and allowed to boil off into a funnel, into which the muzzle of a rabbit was inserted. After somewhat prolonged use, there was not any abolition of reflex action, and the animal almost immediately recovered. The only effect was slight drowsiness.

Ethyl chloride (C_2H_5Cl) boiling at 12 deg. C. = 53·6 deg. Fahr., administered to rabbits in the same way as the above, produced rapid anæsthesia; but in one case the respirations soon stopped, and in another, when air was admitted more freely, general convulsions occurred.

Nitrous ethyl ether ($C_2H_5NO_2$) produced great excitement and convulsions, almost immediately followed by cessation of respiration.

It is obvious that none of these substances would take the place of chloroform, but the committee experimented with isobutyl chloride and ethidene dichloride, with which they obtained comparatively successful results. They thus describe their experiments with these agents:—

Isobutyl chloride (C_5H_9Cl).—1. Experiments on the frog. When it was administered under a glass jar, complete anæsthesia occurred in about five minutes. The heart was observed for thirty-five minutes, during which period its contractions were perfectly vigorous.

2. Experiments on rabbits. When it was administered with a cloth, anæsthesia was produced in three to five minutes. It was continued after anæsthesia for nearly half-an-hour without any interference with respiration.—3. Experiments on dogs. It was administered on cloth; anæsthesia was produced in four minutes. It was continued for half-an-hour, and respiration was unaffected, except slight occasional stertor.

Ethidene dichloride ($C_2H_4Cl_2$, an isomeride of ethene-dichloride, produced from aldehyde). The committee have administered dichloride of ethidene to six patients in the Western Infirmary of Glasgow, with most satisfactory results. The first case was that of a young man aged eighteen, who had an abscess opened near the hip-joint. The anæsthetic was given at 9.19 a.m.; at 9.27, there was complete anæsthesia; the operation began at 9.29, and was completed at 9.32, the patient waking up at 9.35. The recovery was rapid, and there was no vomiting, sickness, or headache. About half an ounce of the substance was used.

In the second case a girl aged thirteen was operated upon, who had abscesses in the thigh, from spinal disease. One abscess pointed towards the outer aspect of the thigh, while another was more in front. The opening of the first was the special operation at this time. The notes record that, before administration of the anæsthetic, the pulse was 134. At 9.47 the anæsthetic was given, and at 9.50 the pulse had fallen to 120; at 9.54 it was 100, and at 9.55 anæsthesia and muscular relaxation were complete. At 9.56½, there was vomiting, which was undoubtedly caused by the patient having had food in the morning, contrary to orders. The operation was over at 10.1, and at 10.3 the patient was recovering. At 10.5½, she was able to answer intelligently. The amount used was three drachms and a half.

Case III. was a girl aged eight. The operation was removal of the forefinger for strumous disease of bone. At the beginning the pulse was 120; in one minute and a half, it had fallen to 110; in two and a half, to 105; in five, to 102; in six and a half, to 100; and in seven minutes anæsthesia was complete. At eight minutes and a half from the time the anæsthetic was given, the breathing became slightly stertorous. At the end of fourteen minutes, the operation was over, and the pulse was 105. The patient recovered from the anæsthetic, so as to answer questions, in about two minutes, and there was no headache, sickness, or vomiting afterwards.

Case IV. was the same patient as in No. 2. The abscess in front of the thigh previously alluded to was opened. At 9.9 a.m., before the anæsthetic was given, the pulse was 140; at 9.10½, it had fallen to 110; at 9.12, it was at 104; at 9.13, there was slight flushing of the face; at 9.14, the pulse was 100, and there was slight stertor; and at 9.15 there was complete relaxation and absence of reflex movements. The operation was quickly over; at 9.22, the pulse was 110; at 9.23, consciousness had returned; and at 9.24, the pulse had risen to 130. The amount used was four drachms.

Case V. was that of a very powerful young man, aged twenty-one, who was operated upon for the radical cure of inguinal hernia. Immediately before giving the anæsthetic, the pulse was very rapid from excitement—about 140. At 9.17, the anæsthetic was given; at 9.18½, the pulse had fallen to 110; at 9.22, pulse 110 and slight struggling; at 9.23, it was observed that the face was slightly flushed; at 9.24 and 9.26, considerable struggling; at 9.28 the pulse had fallen to 74, and the operation was commenced; at 9.35, pulse 70, and respirations deep and regular; at 9.40, the operation was over, and at 9.42 consciousness returned. About 9.41, there was deep anæsthesia and complete muscular relaxation; a condition specially necessary for such an operation. The amount used was an ounce and a quarter.

In ase VI. a little girl aged eight had several pieces of diseased bone removed from the ankle. The anæsthetic was given at 10.3½; at 10.5½, the patient was "over," and the operation was begun. After 10.10, no anæsthetic was given. She quickly recovered consciousness, when there was some retching. It appears that this girl had been ailing with sickness and occasional retching for several days previously. The amount used was about two drachms.

Such is a short record of the six cases observed by the committee. The features of special interest in these cases are the facts that there was no injurious effect observable on the respiratory mechanism, although in all cases the anæsthetic was given in such doses as to produce complete anæsthesia and muscular relaxation, and in one the patient was deeply under its influence for twenty-five minutes. 2. The pulse diminished in frequency and increased in volume, and in the deepest anæsthesia it was steady, regular, full, and compressible. There was no indication of failure of cardiac action in any case: a result anticipated from what had been previously observed in animals. 3. There was never any pallor of the countenance or blueness of the lips, but, on the contrary, and even during the deepest anæsthesia, there was a healthy flush on the face and the lips were rosy-red. Taking into account the change in the character of the pulse and in the colour of the face, it would appear that, in anæsthesia from dichloride of ethidene, the blood still remains in a normal amount in the arterial and capillary systems, and does not tend to engorge the venous system and right side of the heart, as is apparently the physiological action of chloroform.

The experiments on dogs show that a dog will live for a lengthened period in a state of complete anæsthesia under the influence of ethidene dichloride, whilst it will die in a short time when chloroform is used.

Attention is drawn to the fact, which is certainly remarkabe, that butyl chloride and isobutyl chloride, having the same chemical formula, exhibit such different actions. Ethene dichloride and ethidene dichloride are also isomeric, but the first of these produced severe convulsions, while the second promises to be an excellent anæsthetic without any convulsive effects.

Several curious effects, interesting especially from a psychological point of view, have been elicited with regard to the effects of small doses of chloroform and ether on the rapidity of nervous and mental processes. By a refined method of experimenting with Regnault's chronograph, it was ascertained that a few respirations of air containing chloroform or ether produced remarkable retardation in the time of signalling back that a visual impression had been perceived, although the person operated on was quite unconscious of any such delay.

The committee are continuing their researches, and we shall look forward to the issue of their third report. There would seem to be little doubt of their finding an agent or agents which will partially, if not entirely, supersede chloroform as an anæsthetic.

Writing on this subject in the *British Medical Journal*, Mr. J. T. Clover, F.R.C.S., says that a few years ago he received a supply of the ethidene from Messrs. Schoelensack; and after using about half he put the remainder on the shelf, not finding it sufficiently better than chloroform to counterbalance the objections to introduce a new anæsthetic. He found that it produced sickness; and he was not so much impressed by its not seriously depressing the action of the heart, because he has found that chloroform in diluted doses rarely does so.

After reading the report, he found half a bottle (about eight ozs.) of his original stock left. The cork was as sound as if it had only just been put into the bottle, and the liquid was unaltered, thus affording proof of the stability of the substance. He has tried it again, and although probably it excites as much vomiting as chloroform, he is disposed to give it a further trial.

With regard to the remark of the committee that practically a dog will live for a lengthened period in a state of complete anæsthesia under the influence of ethidene dichloride, whilst it will die in a short time when chloroform is used, Mr. Clover suggests that the strength of the vapour of chloroform was greater than that of the ethidene, which would be greatly reduced in consequence of the coldness of the liquid produced by evaporation. He thinks a like failure of the heart might have happened if a fresh bottle of ethidene had been used instead of chloroform.

IX. ON THE TRANSMISSION OF POWER BY MEANS OF ELECTRICITY.

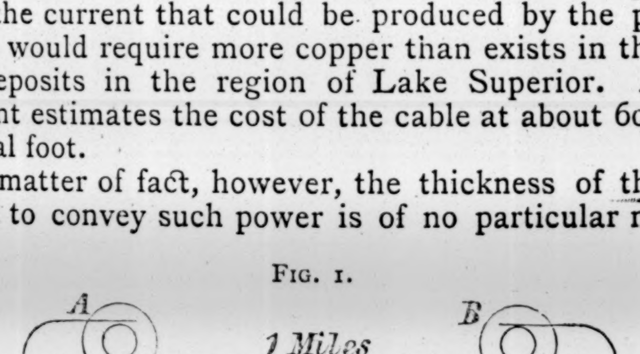
By Profs. ELIHU THOMSON and EDWIN J. HOUSTON.

THE statements recently made as to the size and cost of the cable that would be needed to convey the power of Niagara Falls to a distance of several hundred miles by electricity have induced the authors to write the present paper, in the hope that it may throw light upon this interesting subject.

As an example of some of the statements alluded to we may cite the following, viz., that made by a certain electrician, who asserts that the thickness of the cable required to convey the current that could be produced by the power of Niagara would require more copper than exists in the enormous deposits in the region of Lake Superior. Another statement estimates the cost of the cable at about 60 dollars per lineal foot.

As a matter of fact, however, the thickness of the cable required to convey such power is of no particular moment. Indeed it is possible, should it be deemed desirable, to convey the total power of Niagara, *a distance of* 500 *miles or more, by a copper cable not exceeding one-half of an inch in thickness*. This, however, is an extreme case, and the exigencies of practical working would not require such restrictions as to size.

FIG. 1.



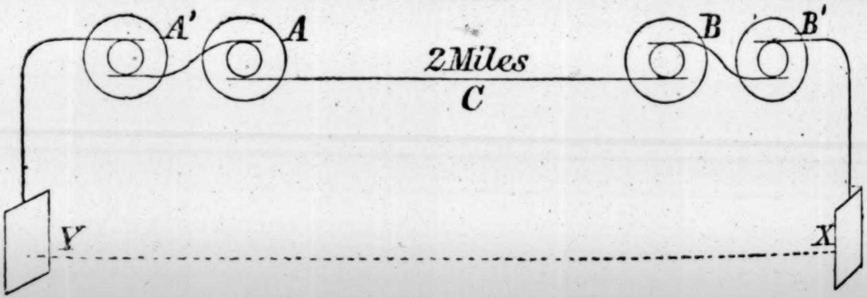
The following considerations will elucidate this matter:— Suppose two machines connected by a cable, of say 1 mile in length. One of these machines, as, for example, A, Fig. 1, is producing current by the expenditure of power; the other machine, B, used as an electrical motor, is producing power, by the current transmitted to it from A, by the cable C. The other terminals, *x* and *y*, are either put to earth or connected by a separate conductor.

Let us suppose that the electromotive force of the current which flows is unity. Since by the revolution of B, a counter-electromotive force is produced to that of A, the electromotive force of the current that flows is manifestly the difference of the two. Let the resistance of A and B together be equal to unity, and that of the mile of cable and connections between them the 0·01 of this unit. Then the current which flows will be—

$$C = \frac{E}{R} = \frac{1}{1{\cdot}01}.$$

If now an additional machine, A′, Fig. 2, an additional motor, B′, and an additional mile of cable, be introduced into the above circuit, the electromotive force will be

FIG. 2.



doubled, and the resistances will be doubled, the current strength remaining the same as—

$$C = \frac{E}{R} = \frac{1+1}{1{\cdot}01+1{\cdot}01} = \frac{2}{2{\cdot}02}.$$

Here it will be seen that the introduction of the two additional machines, A′, B′, has permitted the length of the cable, *c*, to be doubled, without increasing the strength of the current which flows, and yet allowing the expenditure of double the power at A A′ and a double recovery at B B′ of power, *or, in other words, a double transmission of power without increase of current.* Increase, now, the number of machines at A to say one thousand, and of those at B in like proportion, and the distance between them, or the length of the cable, one thousand, or in the case we have supposed, make it 1000 miles, its diameter remaining the same. Then although the same current will flow, yet *we have a thousand times the expenditure of power at one end of the cable, and a* *thousandfold recovery at the other end, without increase of current.* And the same would be true for any other proportion.

Since the electromotive force is increased in proportion to the increase of power transmission, the insulation of the cable and machines would require to be proportionally increased.

As an example, it may be mentioned that a dynamo-electric machine used for the purpose of A in the figure, may have a resistance of say 40 ohms, and produce an electromotive force of say 40 volts. Such a machine might require from three to five horse-power when used in connection with a suitable motor, B, for recovery of the power transmitted.

If the resistance of the motor B be say 60 ohms, and the cable transmitting the currents a distance of 1 mile be 1 ohm, then the current—

$$C = \frac{400}{60+40+1} = \frac{400}{101}.$$

If, now, one thousand machines and one thousand motors, and a thousand miles of cable, each of the same relative resistances, be used, the current—

$$C = \frac{1000 \times 400}{1000 \times 101},$$

which has manifestly the same value as before. If our supposition of the power used to drive one machine be correct, then from 3000 to 5000 horse-power would be expended in driving the machines, and possibly about 50 per cent of this amount recovered. Then we have from 1500 to 2000 horse-power conveyed a distance of 1000 miles. What diameter of copper cable will be required for such transmission? Since this cable is supposed to have the resistance of 1 ohm to the mile, calculation would place the requisite thickness at about $\frac{1}{4}$ inch. If, however, the distance be only 500 miles, then the resistance per mile may be doubled, or the section of the cable be decreased one-half, or its diameter will be less than one-fifth of an inch.

For the consumption of 1,000,000 horse-power a cable of about 3 inches in diameter would suffice under the same conditions. However, by producing a much higher electromotive force, the section of the cable could be proportionally reduced, until the theoretical estimates which we have given in the first part of this paper might be fulfilled. The enormous electromotive force required in the above calculation would, however, necessitate such perfect insulation of the cable that the practical limits might soon be reached. The amount of power required to be conveyed in any one direction would of course be dependent upon the uses that could be found for it, and it is hardly conceivable that any one locality could advantageously use the enormous supposed power we have referred to.

Stripped of its theoretical considerations, the important fact still remains that with a cable of very limited size an enormous quantity of power may be transferred to considerable distances. The burning of coal in the mines, and the conveyance of the power generated by the flow of rivers, may therefore be regarded as practicable, always, however, remembering that a loss of about 50 per cent will be almost unavoidable.

It may be mentioned that Dr. C. W. Siemens and Sir William Thomson have recently made statements that are in general accordance with the views here expressed.

NOTICES OF BOOKS.

Journal of a Tour in Morocco and the Great Atlas. By **Joseph Dalton Hooker**, Pres. R.S., and **J. Ball**, F.R.S. With an Appendix including a Sketch of the Geology of Morocco, by **G. Maw**, F.L.S., F.G.S. London: Macmillan and Co.

There is perhaps no country in existence so near to the centres of European civilisation, and yet so little known, as the empire of Morocco. Its geology, its flora and fauna, and even the main features of its physical geography, are to a very great degree uncertain. The higher interest, therefore, attaches to the explorations undertaken by the authors. The main object of the journey was of course botanical. It had long been the wish of Sir J. D. Hooker to explore the range of the Great Atlas, "up to that time little better known to geographers than it was in the time of Strabo and Pliny, and especially to find whether its vegetation formed a connecting-link between that of the Mediterranean region and the flora of the Canaries and Madeiras." For this task the authors were specially qualified by an acquaintance with the vegetation of Spain, Italy, Asia Minor, Syria, and Algeria. As an indispensable preliminary to travelling in such a region they had been furnished from the Foreign Office with a special recommendation to the Emperor, and they received every possible assistance from Sir J. Drummond Hay, at that time British Ambassador to the Moorish Court. Still their task was by no means easy. The provincial authorities received them, indeed, in deference to the Imperial mandate, with much courtesy, but at the same time showed no little suspicion, and were strongly desirous to prevent them from penetrating to the points they most desired to reach. The Moors, like many persons in our own country, cannot understand a man pursuing knowledge for its own sake. Hence the authors were obliged to represent themselves as searching for plants of medicinal value. Geological and mineralogical research could only be indulged in by stealth, as any one found breaking off fragments of stone is suspected of being a treasure-seeker. How a collector of animals would have to justify his actions to native curiosity is hard to imagine. In this department, however, the travellers do not appear to have taken any special interest. Indeed the fauna of North-western Africa is far from rich. Insect-life was found remarkably scarce, only one species of butterfly (*Papilio podalirius*) being mentioned. Locusts, however, were plentiful and destructive, though their ravages were by no means so indiscriminating as is often represented, certain plants escaping untouched. They are most greedy whilst still young, becoming sluggish when mature. If a swarm invades the fields before the middle of April, when the rainy season is not over, the plants that have been devoured almost down to the root revive with wonderful rapidity. If the swarms appear late, and attack the wheat or maize after it has flowered, the consequences are very serious. In combatting this evil the Moors would be greatly benefitted by the ingenious appliances lately devised for this purpose in the United States.

Certain of the serpents are said to be venomous, the bite of one species—not named—being reputed fatal in two minutes; but no case came under the observation of the travellers. In the Sous Valley, according to tradition, pythons of from 20 to 30 feet in length still lurk—descended doubtless from that monster which encountered the army of Regulus. Birds are not very numerous, but their fearlessness in presence of man is mentioned as an interesting fact.

The flora of Morocco, like its fauna, is far from showing that admixture of semi-tropical and tropical forms which might have been expected. The view of Mr. Wallace—that the Great Desert forms the northern boundary of the Ethiopian region, whilst the Mediterranean coast organically speaking pertains to Europe—appears to hold good in a botanical as well as in a zoological point of view. The Straits of Gibraltar must, however, have divided the two continents for a very considerable time, since, in spite of a general resemblance between the floras of Spain and of Morocco, each has a great number of distinct species. The connecting-links between the vegetation of the latter country and that of the Canaries are unimportant. Still these islands contain some peculiar forms more closely allied to endemic Moroccan species than to those of any other country. They contain also a large class of plants not hitherto found in Morocco, but more closely allied to Mediterranean forms than to any others. Hence Sir J. D. Hooker is disposed to regard these Atlantic islands "as a very distinct sub-region of the Mediterranean province.

The flora of Morocco, and secondarily its fauna, like those of many other countries, has suffered from fire-raising. The herdsmen, to obtain a fresh growth of grass for their cattle, set fire to the thickets, and in this manner the ancient forests of Mount Atlas have been gradually destroyed. A still worse enemy are the goats. Our authors most justly say—"The young plants while yet seedlings are cut off by the teeth of the goat, the great enemy of tree vegetation—an animal whose disastrous influence acting indirectly on the climate of wide regions entitles it to rank as one of the worst enemies of the human race."

The fertility of Morocco, were the land intelligently cultivated, would be prodigious. Even now the Aguidal garden at the city of Morocco, a plot of about 40 acres, yields an average yearly return of £20,000.

The climate is remarkably genial and pleasant. The temperature fluctuates less than that of Madeira, the difference between the average temperature of the hottest and the coldest months not exceeding 10° F., whilst the number of rainy days at Mogador is only forty-five. Pulmonary affections are here all but unknown, and the region wants merely a settled Government to become an admirable sanitarium.

We regret that we must here cut short our notice of a work which literally teems with interesting facts. We can confidently recommend it to all who love accounts of travel, even if they are utterly unacquainted with botany.

The volume is tastefully got up, and is enriched with characteristic views of scenery, and with a map of Southern Morocco, corrected according to the observations of the travellers, and showing their route.

An Illustrated Dictionary of Scientific Terms. By WILLIAM ROSSITER. London and Glasgow: W. Collins, Sons, and Co. 1879.

MR. ROSSITER has tried to compress too large an amount of information into too small a compass, and in numerous instances he has sought for his definitions in untrustworthy directions, the consequence being that a number of words that are found every day in books and newspapers are of necessity omitted, while some of the explanations are calculated to mislead the student. Columbium is not a rare *mineral* found in columbite; coal brasses are not spathic ore; caviare is not a sauce; the papyrus is not a *tree* found on the banks of rivers; and soot is only in a very restricted sense "the unburnt remains of fuel." Had the generally useless cuts been left out Mr. Rossiter might have found room for such words as eosin, quantivalence, phonograph, microphone, &c. A large number of the words, too, are quite out of place in a dictionary of this kind. Surely it is not the office of a scientific dictionary to tell people the meaning of such words as bayonet, beet-root sugar, explosion, gable, language, and so on, all of which they can find in any sixpenny Johnson. This book is not at all worthy of a place by the side of many excellent scientific text-books issued by the same firm.

Coal; its History and Uses. By Professors GREEN, MIALL, THORPE, RUCKER, and MARSHALL, of the Yorkshire College. London: Macmillan and Co. 1878.

A SERIES of Ten Lectures delivered by the Professors of the Yorkshire College at Leeds and Keighley, in accordance with a suggestion made by Dr. Carpenter, the Secretary of the Gilchrist Educational Trust. These Lectures were afterwards revised, put together in a consecutive form, and illustrated with nearly sixty woodcuts. The subjects treated of are—the Geology of Coal, by Prof. Green; Coal Plants and Animals, by Prof. Miall; the Chemistry of Coal, by Prof. Thorpe; on Coal as a Source of Warmth and Power, by Prof. Rücker; and, lastly, on the Coal Question, by Prof. Marshall.

Science made Easy. A Series of Ten Familiar Lectures on the Elements of Scientific Knowledge most required in Daily Life, &c. Parts I. to VI. By THOMAS TWINING, Author of "Technical Training." London: Hardwicke and Bogue.

THE first four parts of this work, which originally appeared in 1871, having speedily run out of print, Mr. Twining has taken the opportunity afforded him by the publication of Parts V. and VI. to thoroughly revise them, and bring down the knowledge contained in them to the present year by adding a postscript to the reprint of each of the four parts. The discoveries of MM. Pictet and Cailletet in relation to the liquefaction of oxygen, hydrogen, and nitrogen, De la Bastie's method of making toughened glass, and several others are noticed in their proper places. In other respects but little change has been made in the work. The last two parts contain four Lectures, entitled—A Glance at the Mineral Kingdom, with Notions concerning the Vegetable Kingdom; Outlines of the Animal Kingdom; and, lastly, a double Lecture on the Outlines of Human Physiology. The diagrams to this part of the work are drawn upon a black ground, which gives them an appearance of relief that is a very great help to the understanding. This is especially apparent in the diagrams relating to cell structure. The experiment was made by Mr. Twining in instituting Working Class Examinations in different parts of the country; and a programme is now issued yearly from the Economic Museum, Twickenham, giving full particulars of the regulations, certificates, and prizes, as well as the facilities offered to Institutions in the metropolis for the delivery of the Course. We are glad to see that Mr. Twining's praiseworthy and intelligent exertions in the cause of the spread of scientific truth amongst the working classes are meeting with such success.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. iv., No. 4. Washington: Government Printing-Office. 1878.

THIS issue contains Prof. S. H. Scudder's account of the fossil insects of the Green River shales, Wyoming. The bulk of the specimens found are damaged beyond recognition. The eighty species identified belong to six different orders of true insects, and have a decidedly tropical character.

Dr. S. Jordan reports on the collection of fishes made in Dakota and Montana by Dr. Elliott Coues, and Prof. Chickering gives a catalogue of 692 species of phanerogams and vascular cryptogams collected in the same territories. This document is of great value, from the light it throws upon the range of many plants.

Dr. Endlich's memoir, "On some Striking Products of Erosion in Colorado," is exceedingly instructive, and should be read by those—for such persons still exist—who fancy that the beds of rivers have been made not by them, but for them.

Mr. J. A. Allen contributes a paper on the *Sciuri*, or tree-squirrels, of America.

Records of the Geological Survey of India. Vol. xi., Part 3. 1878.

THIS issue contains an account of the progress of the Gold Industry in Wynad, in the Nilgiri district. Three companies have been at work for some time, not in the most scientific manner, but until recently the returns have not been very favourable. It is only within the last few months that some stone from "Wright's level," at the Alpha Works, has yielded from 11 to 17 dwts. per ton. From the latest reports of the working of the Prince of Wales' Company a rich vein has at length been struck.

Mr. F. R. Mallet describes a curious antimony ore from Sarawak, consisting partly of senarmontite.

Botany; Classification of Plants. By W. Ramsay McNab, F.L.S. "The London Science Class-Books," edited by G. Carey Foster, F.R.S., and P. Magnus, B.Sc. London: Longmans and Co.

In this little manual compression seems to have reached, if not exceeded, its fair limit. Reversing the plan followed by Prof. McAlister in the companion volumes on zoology, the author devotes the larger portion of the space at his disposal to the lower forms of plant-life. Among the Thallophytes the families are characterised, whilst among phanerogamous plants they are simply enumerated without any reference to their distinctive features. In consequence the two portions of the book are of unequal value. There is a table of contents, but no index—an unfortunate deficiency, though inevitable from want of space.

Annual Report of the Board of Regents of the Smithsonian Institution, showing the Operations, Expenditure, and Condition of the Institution for the Year 1877. Washington: Government Printing-Office.

This annual volume contains, in the first place, an account of the work done by the Institution and of its present position. The secretary, Mr. Jos. Henry, continues to urge, on what appear to us very valid grounds, the entire separation of the Institution from the National Museum of the United States. This latter establishment was formerly under the charge of the Patent Department, but was transferred to the Smithsonian Institution in 1858, whilst its importance fully warrants a distinct establishment. The Proceedings of the Smithsonian Institution and of the Museum are of course blended in the volume before us.

Amongst the most important work of the year we must mention Dr. Habel's "Archæological and Ethnological Researches in Central and South America." This gentleman, having taken Guatemala as a centre, devoted seven years to an exploration of the region, and afterwards visited Colombia, Ecuador, and Peru.

Nos. 7, 8, and 9 of the Bulletins of the Museum have also been published. The first of these is a valuable contribution to the natural history of the Hawaian and Fanning Islands, and of Lower California. No. 8 is an index to the hitherto very complex synonymy of the Brachiopoda, which rank among the most characteristic forms by which strata are co-ordinated.

Perhaps the most valuable part of the "Report" is the Appendix, which contains a profusion of valuable matter not commonly to be met with. We find, in the first place, a full translation of Prof. Holmgren's memoir on colour-blindness—a subject which is now attracting general attention, from its bearing upon the correct interpretation of signals by railway officials and sailors. For the detection of this defect he gives a decided preference to Seebeck's method, an assortment of Berlin wools serving as test-objects. There is another interesting essay on the same subject, by Mr. Jos. Henry.

Next follow translations from the "Transactions of the Geneva Society of Physics and Natural History." There are, further, very valuable papers on the antiquities and ethnology of various parts of the Western Hemisphere; notes on the "History and Climate of New Mexico," in which that region, on the faith of a considerable body of statistical evidence, is recommended as a sanitarium for the consumptive. Dr. R. H. Coolidge is quoted as saying that "the worst possible climate for a consumptive is one with a long-continued high temperature and a high dew-point." Yet we have heard of not a few cases of incipient consumption where great benefit has been experienced from a residence in Sierra Leone.

Dr. Weismann's paper on the change of the Axoloth to an Amblystoma is given in full, together with an account of the experiments performed by Marie v. Chauvin.

The volume is completed by a number of short meteorological memoirs.

We notice in this "Report," as in former issues, a peculiarity which greatly detracts from its utility. The index gives merely the headings of the memoirs or articles, omitting all details,—a serious defect in a work containing a mass of information at once so important and so varied. We should beg to suggest the publication—say once in ten years—of a general index of subjects.

A Treatise on Chemistry. By H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S., Professors of Chemistry in Owens College, Manchester. Vol. ii., Metals (Part 1). London: Macmillan and Co.

This volume opens with a consideration of the generic properties of the metals. The authors, we find, take a well-founded objection to the term "metalloid," still applied by some writers to non-metallic bodies, and insist on the absence of any distinct line of demarcation between the two great classes of elementary substances, not a few of which are alternately pronounced either metallic or non-metallic, according to the point of view whence they are considered. The remarks on alloys, though necessarily brief, bring into prominence several facts often overlooked. After discussing the specific gravity of metals, their melting-points, atomic heats, and quantivalence, the authors pass to a consideration of the general properties of acids, the constitution of salts, &c. The metals and their compounds are then described in succession, according to the classification given on p. 22.

The present work is one of the very few chemical treatises in which the peculiar and unpleasant odour of potash-lye, so well known to practical men, is mentioned, though no light is thrown on its origin. In treating of the commercial sources of potash-salts, the manufacture of beet-root and cane-sugar is mentioned as a fortunate discovery. We cannot help regarding it as a serious mistake, which, if persevered in, must lead to the exhaustion of the soil.

Sodium and its compounds are described in considerable detail. The great English beds of rock-salt at Northwich, generally said to occur in the new red sandstone, are here described as found in the mountain limestone. It is noted that the process of evaporating brine, as followed in Cheshire, has not been improved since the times of the Romans. The alkali manufacture (Leblanc's process), as now carried on, is very fully described, and admirably illustrated with drawings done to scale. The recent improvements, proposed or actually carried out, are not omitted. Thus we have notices of Deacon's closed roasters for preventing the escape of hydrochloric fumes, of the mechanical salt-cake process of Messrs. Jones and Walsh, of Middlesborough, intended to obviate all hand-labour, and of the salt-cake furnace of Messrs. Cammack and Walker, which is declared to be based upon a more scientific view of the decomposition than any former plans, though at the same time no opinion is expressed as to its practical feasibility. The Hargreaves process for decomposing salt directly with sulphurous acid, in presence of air and steam, is also described. Mr. Mond's process for the recovery of sulphur from the vat-waste is noticed, as also the ammonia-soda process of Dyer and Hemming, which, as latterly improved by Solvay, is now worked on the large scale.

Under Calcium we find a description of the manufacture of bleaching-lime, with an account of Weldon's process, and of the improved chamber devised by Mr. Deacon. We do not see any mention of the revolving process recently invented, and said to give remarkably uniform results.

The authors enter at some length into the metallurgy of zinc, lead, copper, silver, and mercury, giving carefully executed drawings of the requisite furnaces. The electrotyping process is also described, without any reference to the share of the late Mr. Smee in the early development of the art. Electroplating and photography are noticed, though necessarily in brief compass.

Aluminium in the metallic state is a curious instance of a discovery from which great hopes were entertained proving subsequently a practical failure. Two interesting facts not generally known are mentioned in this chapter—the resuscitation of the ancient and renowned alum-works of La Tolfa by a French company, and the cessation of the manufacture of ammonia-alum. Some ten or twelve years ago it was often difficult to meet with a sample of potash-alum.

The volume before us merits and will receive the same favourable reception which was awarded to its predecessor. To the chemical student it offers the great advantage of bridging over the gulf between theory and practice better than probably any other text-book in the language. To manufacturers, foremen, &c., it would be of equal service for the same purpose were it not for the exclusive use made of the so-called "new" nomenclature. Had it been desired to keep practical men in the dark concerning chemical principles, and to produce confusion by the multiplication of synonyms, we doubt if a more successful method could have been adopted than this change of terminology.

CORRESPONDENCE.

THE BAND-PATTERN IN ANIMALS.

To the Editor of the Monthly Journal of Science.

Sir,—May I ask if any readers of the "Journal of Science" have made observations bearing on the curious fact that in a multitude of species belonging to different parts of the animal kingdom the colours are arranged, in bands or stripes, approximately at right-angles to a line drawn from the head to the tail? Further, in a vast majority of cases, these bands are alternately yellow and black, or at least a very deep green, deep purple, &c. This pattern prevails, among mammals, in the tiger and the zebra; among Hymenopterous insects, in a number of bees and wasps. But among the Coleoptera it recurs in a multitude of groups differing widely in their habits, localities, and affinities. Thus among the so-called "long-horns" we find it in the genera *Clytus*, *Rhagium*, *Stenura*, and *Diastocerus*; among Buprestids it occurs in *Cyria*, *Diadoxus*, *Nascio*, *Conognatha*, *Stigmodera*, and *Buprestis*. Among the Cetoniadæ it is met with in *Plæsiorhina*, *Anisorrhina*, *Allorrhina*, *Clinteria*, *Gametis*, *Pachnoda*, and *Euphoria*. It may finally be traced among the highly carnivorous tiger-beetles (*Cicindela*), and in the carrion-feeding sexton-beetles (*Necrophorus*). In all these cases, save the moths, the bands consist alternately of yellow, orange, or buff, and of black or some very dark shade. A few of these instances, such as those of the tiger and zebra, may subserve the purpose of concealment; and others, *e.g.*, *Clytus arietis*, may be mimetic, causing the insect to be mistaken for some more formidable species. But in the majority it seems as if we encounter here the markings of some more deeply-hidden law.—I am, &c.,

J. W. S.

THE SEA-SERPENT.

To the Editor of the Monthly Journal of Science.

Sir,—I have read with much interest your notice of Dr. Wilson's memoir on sea-serpents, and quite agree with you and with him that the question of their existence should be fairly and dispassionately discussed by biologists. With your permission, however, I will make a few remarks on the case of the *Pauline*. If the narrative deposed to at Liverpool is literally true, all doubt is at an end. No giant seal, basking-shark, piece of floating wreck, sea-weed, or flight of birds could act as the unknown monster is said to have done. The testimony—that of a number of disinterested observers who watched the struggle for a quarter of an hour—is such as would be accepted in any court of justice in the most serious case. Still there are certain points on which further light would be very welcome. When a boa or python attacks a deer its dry scaly surface is pressed against the rough hairy coat of the quadruped, whose limbs, further, prevent the deadly coils from slipping from their place. The sea-serpent, on the contrary, has to apply its skin, probably slimy, to the exceedingly slippery surface of the whale, where there is no neck, waist, or projecting limb to aid in retaining the folds. Would not the difficulty of killing by constriction be greatly enhanced under such circumstances? Again, the serpent whilst grappling with the whale is described as having its own head and tail disengaged, to the extent, as I understand, of 30 feet at each end. Now non-poisonous serpents, in attacking, invariably either keep hold of some fixed object with their tail, or lay hold of the intended victim with their jaws. As no fulcrum for the tail was possible in this case, I should think that a preliminary seizure with the mouth would be all the more essential. That the jaws must be capable of opening wide enough for this purpose is evident if the prey was afterwards to be swallowed. A "large" sperm whale measures in girth about 17 metres. It is much to be regretted that no naturalist appears to have had a conversation with the crew of the *Pauline*.—I am, &c.,

SERPENT-HUNTER.

PROCEEDINGS OF SOCIETIES.

Royal Society, *January* 9.—The first paper was a "Note on the Inequalities of the Diurnal Range of the Declination Magnet as recorded at the Kew Observatory," by Balfour Stewart, F.R.S., and William Dodgson. The authors are at present engaged in searching for the natural inequalities of the above range, more especially for any of which the period is between 24 and 25 days. They have found strong evidence of an inequality of considerable magnitude of which the period is 24·00 days, very nearly. They have also found preliminary evidence of the existence of two considerable inequalities having periods not very far from 24·65 and 24·80 days. These two appear to come together in about eleven years, but the exact time of this cannot be given. No trace of any inequality with a period of 24·25 days has been found. We shall give a report of a more recent paper on this subject in our March number.

"Some Experiments on Metallic Reflexion," by Sir John Conroy, Bart., M.A. The author finds that when light is reflected from a polished surface of gold or copper in contact with various media, the angle of principal incidence diminishes, and the principal azimuth increases with the increase of the refractive index of the medium in contact with the metallic surface; and further, the diminution in the value of the principal incidence appears to be nearly in proportion to the increase of the refractive index of the surrounding medium. The values of these angles for gold with red light are—

	Principal Incidence.	Principal Azimuth.
In air	76° 0′	35° 27′
In water	72 46	36 23
In carbon bisulphide ...	70 03	36 48

Assuming that the angle of principal incidence for a metal is the same as the angle of polarisation of a transparent substance,—that is, the angle whose tangent is equal to the refractive index,—the value of that angle in air, as deduced from the measurements made in water and carbon bisulphide by multiplying the tangent of the principal incidence in those media by their refractive indices, is 76·53 and 77·22 instead of 76.

"Researches on the Absorption of the Ultra-Violet Rays of the Spectrum by Organic Substances," by W. N. Hartley, F.Inst. Chem., F.R.S.E., F.C.S., and A. K. Huntingdon, F.Inst. Chem., A.R.Sc. Mines, F.C.S. The authors find that—

1. The normal alcohols of the series $C_nH_{2n}+_1OH$ are remarkable for transparency to the ultra-violet rays of the spectrum, pure methylic alcohol being as nearly so as water.
2. The normal fatty acids exhibit a greater absorption of the more refrangible rays of the ultra-violet spectrum than the normal alcohols containing the same number of carbon atoms.
3. There is an increased absorption of the more refrangible rays corresponding to each increment of CH_2 in the molecule of the alcohols and acids.
4. Like the alcohols and acids, the ethereal salts derived from them are highly transparent to the ultra-violet rays, and do not exhibit absorption-bands.

In order to ascertain whether isomeric bodies exhibited similar or identical absorption-spectra, a series of benzene derivatives was examined. The results may be summarised as follows:—

1. Benzene, and the hydrocarbons, the phenols, acids, and amines derived therefrom, are remarkable—first, for their powerful absorption of the ultra-violet rays; secondly, for the absorption-bands made visible by dissolving them in water or alcohol, and diluting; and thirdly, for the extraordinary intensity of these absorption-bands,—that is to say, their power of resisting dilution.
2. Isomeric bodies, containing the benzene nucleus, exhibit widely different spectra, inasmuch as their absorption-bands vary in position and in intensity.
3. The photographic absorption-spectra can be employed as a means of identifying organic substances, and as a most delicate test of their purity. The curves obtained by co-ordinating the extent of dilution with the position of the rays of the spectrum absorbed by the solution form a strongly-marked and often a highly characteristic feature of many organic compounds.

There is a curious feature in connection with the position of the absorption-bands; at the less refrangible end they either begin at line 12 Cd or line 17 Cd, and those which begin at 12 end a little beyond 17.

"On the Electromagnetic Theory of the Reflection and Refraction of Light," by George Francis Fitzgerald, M.A., Fellow of Trinity College, Dublin. The author shows that the method adopted in his former paper on Magnetic Reflection, in the "Proceedings of the Royal Society" for 1876 (No. 176), is justified, and that it is legitimate to consider an incident plane polarised ray as composed of two oppositely circularly polarised rays, each of which is reflected according to its own laws. He considers next the cases of the magnetisation being all normal to the surface, and all in the surface and the plane of incidence, and obtains the following result:—When the incident ray is plane polarised, and the plane of polarisation is either in or perpendicular to the plane of incidence, the effect of magnetisation is to introduce a component into the reflected ray perpendicular to the original plane of polarisation, which vanishes at the grazing and normal incidences, and, in the case of iron, attains a maximum at about the angle of incidence $i=63°20'$. He does not obtain any change of phase by reflection in any case. Apart from this question of change of phase, his results conform completely to Mr. Kerr's experiments on the reflection of light from the pole of a magnet.

CHEMICAL SOCIETY, *January* 16. — Dr. J. H. Gladstone, F.R.S., President, in the chair.

The first communication was made by W. H. Perkin, F.R.S., "On the Action of Isobutyric Anhydride on the Aromatic Aldehyds." The author has studied the reaction which takes place when isobutyric is substituted for butyric anhydride.

The next paper was by Dr. Dupré and H. Wilson Hake, "On Two New Methods for the Estimation of Minute Quantities of Carbon: (1) Gravimetric; (2) Chromometric; and their Application to Water Analysis." (1) The Gravimetric Method:—This method consists essentially in burning the small quantity of carbon in a stream of oxygen in an ordinary combustion-tube containing some granulated cupric oxide. The method is found to be quite accurate enough for determining the carbon obtained from a litre of a first-class potable water. The chromometric method, or, as the authors name it, the nephelometric method, consists essentially in burning the carbon, but the carbonic acid is conducted into a standard solution of basic acetate of lead, and estimating the turbidity produced, as compared with that produced by the carbonic acid evolved under similar circumstances by a known and nearly equal quantity of carbon, the difference between the two being estimated by a Mills's colorimeter. This method is extremely delicate, and a difference produced by 3·100ths of a milligrm. can be clearly estimated. The authors suggest their use for estimating the carbon in iron and the carbonic acid in air.

Dr. Frankland read a communication "On Stannic Ethide," by E. Frankland and A. Lawrance. In endeavouring to prepare stannous ethide by the action of zinc ethyl upon dry stannous chloride the authors discovered that the reaction was—

$$2SnCl_2+4ZnEt_2=Sn+SnEt_4+4ZnEtCl,$$

and that by its means stannic ethide could be prepared more conveniently and in larger quantities than has hitherto been possible.

The next paper was by R. S. Dale and C. Schorlemmer, "On Aurin," Part II. In previous researches the authors stated that by the action of alcoholic ammonia on aurin, rosanilin was obtained, and a dilemma arose, that if aurin had the formula $C_{20}H_{14}O_3$, rosanilin could not have the formula which Hofmann had proved it to have, $C_{20}H_{19}N_3$. The authors therefore again prepared pure aurin and confirmed their previous analysis. Zulkowsky states that the method employed by the authors yields wretched results, and cannot be used on the manufacturing scale. The authors, however, affirm in the present paper that under certain conditions, which they are not at liberty to divulge, a fairly good yield can be obtained. They have used two methods to purify aurin; one by conversion into ammonia aurin and the decomposition of the latter by hydrochloric acid, and a second by taking advantage of the fact that the solubility of aurin in alcohol decreases with the removal of the by-products. The analyses of pure aurin thus obtained completely confirmed the formula $C_{20}H_{14}O_3$. Some beautiful specimens were exhibited in connection with this paper.

A paper was read by W. Carleton Williams "On the Derivatives of Di-isobutyl."

The last paper was "On the Action of Chlorine upon Iodine," by J. B. Hannay. The author has re-examined the question as to the existence of the compound ICl_4, and has come to the conclusion that such a body has no existence, for two reasons. First, that the reaction for its formation is impossible, as no high chloride of iodine can exist in the presence of free iodine; and, secondly, that careful experiments by which chlorine is added to iodine in the most advantageous manner for the formation of a high chloride fail to indicate such a body,

METEOROLOGICAL SOCIETY, *January* 15.—The annual meeting of this Society was held at the Institution of Civil Engineers—Mr. C. Greaves, President, in the chair.

The report of the Council showed that the chief features of the proceedings during the year 1878 had been the final completion, on a comprehensive and well-organised basis, of the arrangements for systematic inspection of the Society's stations—an object which has engaged the sedulous attention of successive Councils for the last four years—and the delivery of a series of lectures on meteorology by certain members of the Council. The total number of Fellows now amounts to 425, 41 having been elected during the year.

The President having delivered his address "On Dryness versus Humidity," the Officers and Council for the ensuing year were elected.

January 18.—The usual monthly meeting of this Society was held at the Institution of Civil Engineers The following papers were read:—Abstract of "The Meteorology of Bombay Presidency," by C. Chambers, F.R.S., communicated by Sir G. B. Airy, K.C.B., F.R.S., Astronomer Royal. "Experiments with Lowne's

Anemometer," by Captain William Watson, F.U.S.; "Meteorology of Bangkok, Siam," by J. Campbell, Staff-Surgeon, R.N.; "Results of Meteorological Observations taken at Calvenia, South Africa," by Kaufmann J. Marks, F.U.S.

PHYSICAL SOCIETY, *January* 15.—Prof. G. C. Foster, Vice-President, in the chair.

Dr. Erck exhibited a constant bichromate of potash battery. It consists of a narrow lead trough, 12 ins. long by 3 ins. wide and 1 in. deep, lined along both sides with the carbon plates. The zinc plate, 10 ins. long, is immersed in the solution to the depth of an inch midway between the two carbons. A continual circulation of the bichromate solution is kept up by allowing fresh solution to drop into the cell at one end, and the exhausted solution to drop away by a tap at the other end. As the space between the two carbons is only about half an inch wide, there is merely a thin layer of solution between the positive and negative poles. The internal resistance of the cell is therefore very low when short circuited, only about ¼ ohm. To obtain the maximum current, about 8 ozs. of solution per hour should be supplied.

Dr. Guthrie, F.R.S., described some of the results he had obtained from experiments on the vibration of metal rods or lathes fixed in a vice at one end and free to vibrate at the other. The experiments were carried on by dusting sand on the rod and observing the nodal lines formed by it when the rod was vibrated so as to give out notes determined by a monochord. Dr. Guthrie's results showed that the two final segments at the free end are equal in length to the inner segment at the fixed end. It appears from these experiments that if a free lathe, vibrating with a node in the middle but having an even number of segments, be clamped at where there is a node we alter its conditions of vibration. When the lathe is half free the end segment breaks up into two parts together equal to the segment at the fixed end. In the case of torsional vibration of the lathe the position of the longitudinal nodal lines depended to some extent on the clamping of the lathe in the vice.

Prof. Foster pointed out that in a natural node the direction of the tangent is varying, whereas in an artificial node it is always horizontal.

Prof. Unwin explained that the sand accumulated at nodes because the particles when thrown off the lathe make certain horizontal excursions, which tend to move them nearer the points of repose of the lathe.

NOTES.

AGRICULTURE.

THE *Phylloxera* Commission of the French Academy have resumed their labours after having suspended them for two years. They have been roused into action by the increased ravages committed on French vines during the last summer and autumn. The Minister of Agriculture has allowed the Commission a certain sum for expenses. Let us hope that their present labours will be more fruitful than their former ones.

BIOLOGY.

The blood of the Octopus contains—instead of hæmoglobin—hæmocyanin, an organic compound containing copper, which here assumes the function fulfilled by iron in the circulation of the Vertebrates.

The chestnut trees of the Cevennes are perishing in consequence of their roots being attacked by a fungus. The tannic acid present in the juices of the trees exudes, and forms black stains on the trunks and on the surface of the soil, whence this new epidemic is locally known as *Maladie de l'encre*.

The celebrated botanist F. Cohn, in speaking of *Anastatica Jerochontica*, said, "The rose of Jericho,"—which, in the first place, is not a rose, and which, secondly, is not found near Jericho.

The well-known butterfly *Colias Edusa* (clouded yellow), which was in 1877 more plentiful in England than ever before, has been last year as remarkably and exceptionally scarce.

M. V. D. Costes, in the "Correspondance Scientifique," suggests that ants have already passed through their stage of social progress, and, having reached the highest perfection consistent with their faculties, have now become stationary, and incapable of further advance.

According to Mr. F. P. Pascoe (Entom. Mo. Mag.) a new species of *Siderodactylus* is ravaging vines in the Island of Ascension. It seems to have been imported from South Africa.

M. L. Fredericq, in a paper communicated to the Academy of Sciences, maintains that the changes of colouration in the skin of the Octopus have no correspondence with the facts of mimetism, and resemble rather the changes produced in the human face by the emotions of fear or anger.

M. Dareste has experimented on the suspension of life in the embryo chicken. He finds that in eggs taken from under a hen after three days' incubation, and exposed to the temperature of the atmosphere, which was then 20° C., the movement of the heart was not completely arrested until after the lapse of seven days.

Haltica nemorum, a tiny yellow-striped beetle, proves to be an enemy to barley.

Prof. Turner, of Edinburgh, in his Memoirs on the "Comparative Anatomy of the Placenta," points out essential differences between the structure of this organ in the sloth and in the lemurs, especially in the *Protopithecus* of Madagascar.

M. Bordier considers atmospheric pressure as one of the principal agents in the transformation of species.

According to M. P. Bert the periodic movements of plants and their heliotropism are due to variations in the quantity of glucose contained in the moving part, in consequence of its state of hydration and its consequent degree of tension.

Prof. Haberlandt considers that the chlorophyll in the cotyledons of *Phasiolus vulgaris* is formed from starch. The starch-granules are gradually surrounded with a layer of protoplasma, which is at first colourless, but gradually turns green, while the starch grains disappear.

M. Yung has communicated to the Academy of Sciences the results of his experiments on the action of different coloured light upon the development of animal ova and larvæ. Their action, beginning with the most favourable, may be arranged thus:—Violet, blue, red, yellow, white, green. The red and green light appeared positively hurtful. Between yellow and white light there was little difference. Tadpoles kept without food died most quickly in the violet and blue rays. The incidental mortality appeared greater in the coloured rays than in white light.

CHEMISTRY AND TECHNOLOGY.

Wines on analysis often display properties not suspected from their taste. Sorts rich in alcohol are often pronounced light, and others strongly acid seem mild, &c. These seeming contradictions are, according to Drs. Mach and Patele, due to the fact that wine is a most complicated liquid, whose components are by no means all known. Even among the recognised and more prominent ingredients, it is not so much the quantity of one or the other as their relative proportion which determines the flavour.

Some new phenomena observed in "plastering" wines and musts are recorded by Prof. E. Pollacci. He finds sulphate of lime reacts solely upon the cream of tartar, producing acid sulphate of potassa, which remains in solution, while tartrate of lime is formed and is chiefly deposited. The reaction between the two salts is not, however, complete, since there may be found in the liquid both sulphate of lime and cream of tartar. If the gypsum is in large excess a part of it is deposited among the tartrate of lime.

Dr. Theodor Schuchardt, of Goerlitz, has introduced a new body, which he calls silicium strontium. It is formed from the preparation of metallic strontium by electrolysis. The compound is a grey powder with a slight odour resembling phosphuretted hydrogen. When mixed with dilute hydrochloric acid a rapid evolution of the spontaneously inflammable siliciuretted hydrogen takes place. If obtainable in quantity this compound will probably be the readiest source of siliciuretted hydrogen.

A ready means of detecting the adulteration of saffron is suggested by Messrs. Domeier and Co. To detect adulteration with *Calendula* flowers (Feminelle) it is merely required to moisten a few flowers, and to rub them singly with the finger on white paper. The genuine flowers will give a fine rich yellow colour, whilst the Feminelle will only yield a violet reddish hue. It can also be easily detected by soaking the suspicious flowers in pure —or, better still, distilled—water. The real saffron will retain its fine red colour after hours, whilst the Feminelle will lose its artificial tint within a short time. To detect an admixture of honey and barytes it is merely required to put a pinch of saffron in a tumbler with pure clear water, agitating it for a few minutes. Adulterated saffron will at once turn the water cloudy, and even small particles of dust may be seen falling to the bottom, which, on pouring the water carefully out, will be found to be a slimy, sand-like mass. With pure saffron the water will remain clear, showing a fine pure yellow colour, which, according to the quality of the flowers, will be more or less intense. Five or ten minutes suffice for these experiments.

METALLURGY.

An admirable sketch of the present position of the French Iron-trade, drawn up by M. S. Jordan, Professor of Metallurgy at the Ecole Centrale, for the use of his foreign colleagues at the Paris meeting of the Iron and Steel Institute, is given in the October number of the "Revue Universelle des Mines." It begins by describing the different French coal-basins. The iron-ores native to France are then described, and the analyses of most of them are given. We are next presented with a series of tables showing the consumption of French ores both native and foreign. The smelting processes also receive attention, and are described in eleven geographical groups.

PHYSICS.

Mr. Louis Schwendler has reported to the India Office on the suitability of the electric light for India. We shall refer at length to this report in our next number; meanwhile we may mention that Mr. Schwendler advocates the employment of the electric light at Indian railway-stations.

The German Government lost no time in the practical employment of the telephone. There are now 272 telephonic circuits, and communication is satisfactorily carried on over a distance of from 30 to 40 miles.

Mr. Stroh, the well-known mechanician, has observed that if a telephone, with the circuit of its coil left open, be held to the ear, and a powerful magnet be moved gently up and down along the length of the magnet, and at a distance of an inch or two from it, a faint breathing sound will be heard; the recurring pulses of sound keeping time with the up and down motion of the magnet. The sound may, according to the "Telegraphic Journal," be aptly compared to the steady breathing of a child, and there is a striking resemblance between it and the microphonic sounds of gases diffusing through a porous septum, as heard by Mr. Chandler Roberts, F.R.S.

Writing on the subject of Gas *versus* Electricity, to "Nature," Mr. W. H. Preece observes that we know no more of the electric light now than we did in 1862, when as great a display was made in our Exhibition of that year as was made in the French Exhibition of last year. The recent experiments have, he says, shown both the strength and weakness of the position of the gas companies. Their strength consists in their being in possession of the ground; their weakness consists in their producing only a poor light—and a very poor light—when compared with electricity. But it would almost seem, from the experiments that have been made, that the quantity of light to be produced by gas is only a question of the quantity of gas consumed in a given space. There are now burning in the Waterloo Road two brilliant gas-lamps, giving a light of 500 candles; and this is greater, in point of fact, than the intensity of the light developed by any one of the electric lights that are now on trial in the thoroughfares of London. There is, however, a defect in gas light which remains to be eradicated, and that is the colour of the light. The one great advantage which the electric light has over gas is that the electric light, owing to its very high temperature, produces rays of every degree of refrangibility, and therefore as an illuminating power it is equal to that of the sun. A very marked advance towards perfection in this direction in gas-lighting has been made in the albo-carbon process, by which the gas burnt is enriched with the vapour of naphthalin—a refuse of gas manufacture. By this process the intensity of the light of a gas-burner is improved at least five times, and in some experiments witnessed by Mr. Preece the improvement was as much as twenty times. There are three points which all electric lights for general purposes should be required to attain: the first is a brilliancy far exceeding that of any known lamp; the second is a durability greater than that which would be required for night operations in England; and the third is absolute steadiness, to enable work to be conducted without affecting the eyes. There is no electric light that has yet been introduced which supplies us with these desiderata.

The first of the Friday evening discourses, at the Royal Institution of Great Britain, was given on the 17th ult., by Professor Tyndall, his subject being "The Electric Light." The electric light had, he said, been known for seventy years, as in 1808, and again in an improved form in 1810, it was shown to audiences at the Royal Institution. Prof. Tyndall quoted Faraday's saying, that he would rather occupy himself with finding fresh effects than spend his time in exalting those effects. But it was the exaltation of these effects which he first studied in a simple way which has led to the present possibilities of our electric lighting. Prof. Tyndall gave a historical sketch of the various arrangements, beginning with that of Mr. Holmes in 1862. The different "candles" in use were also described. Prof. Tyndall said he did not believe any fresh scientific discovery was needed to make the electric light of general application to large places. The scientific man knew what different natures of machines were required to do the different kinds of work to be done. It remained now for mechanical skill to carry out the work. In conclusion he pointed out the mistake of those who, like Cuvier, spoke with contempt of those whose practical skill carried to utility the experiments of the philosopher.

The Philosophical Society of Glasgow have instituted a triennial lecture in memory of the late Prof. Graham, Master of the Mint. The Council have also caused a portrait medal of the value of £10 to be struck, and this they propose to award every third year for the best original investigation, either in chemical physics or in pure or applied chemistry, which may be communicated to the Society or to its chemical section in the interval of the awards. The choice as first Graham Lecturer appropriately fell on Mr. W. Chandler Roberts, F.R.S., who had so long been associated with Graham's work, and who was enabled to employ much of his original apparatus. The lecture was given on the 22nd ult., Mr. James Young, F.R.S., presiding. Mr. Roberts described in detail the experiments that enabled Graham to establish the law of the diffusion of gases, and he illustrated experimentally the passage of gases through porous bodies, such as unglazed earthenware and artificial graphite, as well as through a layer of the hard translucent variety of opal known as hydrophane. Mr. Roberts pointed out that the Graham law of diffusion forms the basis of the science of molecular mechanics, and his measurements of the rates of diffusion prove to be the measure of molecular velocities which have been investigated mathematically by Clerk Maxwell, Clausius, and Boltzmann, and experimentally by Losehmidt in developing the dynamical theory of gases.

The liquefaction of gases formed the subject of Graham's earliest paper in 1826, and it occupied his attention at intervals during his life. He held the view that hydrogen, when absorbed by palladium, is reduced to the metallic form—a supposition which has received strong confirmation from the success that has attended M. Raoul-Pictet's efforts to solidify this gas. At the conclusion Mr. Roberts said the Graham lecture had been instituted in honour of the labours of a life the memory of which will be as enduring as its work, and to stimulate others to investigate as patiently and earnestly the varied phenomenon whose basis is molecular mobility.

Mr. F. H. Ward, F.R.M.S., has endeavoured to get rid of the objectionable black lines across the spectrum caused by dirt adhering to the jaws of the slit, by substituting a right-angled prism with the apex of the right angle carefully ground off and polished, forming what he terms a solid slit. With glass prisms, although it answered well for bright line spectra, it was an entire failure so far as continuous spectra were concerned. The spectrum was as full of lines as with a very dirty slit. The cause was evident upon microscopical examination, the edges being splintered. Better results were obtained with quartz prisms, which promise well when more skill is obtained in polishing the very delicate edge.

The value of telescopes for viewing objects at short distances appears to have been overlooked. The writer frequently employs a telescope for examining plants on cliffs, in ponds, and other inaccessible places at distances as small as 15 ft. The effect is almost microscopic, details of flowers and venation of leaves being clearly brought out. The instrument is a small one by Ross, having an object-glass of 1·35 ins. aperture and about 12 ins. focal length, the ample length of slide allows such near objects to be distinctly focussed; while its more legitimate performance is all that can be desired, and its corrections perfect enough to allow many celestial objects to be seen, bearing well a tolerably deep eye-piece.

THE MONTHLY

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I. THE KEYS OF DEATH.

IS strychnin, or prussic acid, or arsenic a poison? The answer is ready. What is a poison? The oracles are dumb. We have in these days almost ceased playing at the game of definitions. We no longer ask what is gravitation, or motion, or light, feeling that our answers to such questions can only bewilder. Definitions of life are still gravely asked for, and as gravely propounded. But they, too, will soon be given up, and neither Science nor daily life will find cause to regret them. As concerns a satisfactory definition of poison the case is very similar. Two attempts have, however, been lately made to supply this deficiency. Certain physiologists, speaking in the interest of a certain philanthropic movement, contend that the idea of poison is purely qualitative, and that whatever is poisonous in large doses is poisonous also in the very smallest proportions, the only difference being that the injury done may escape observation unless the dose is often repeated. We cannot for a moment accept this view. We maintain that it is perfectly unchemical to ignore the ideas of quantity and concentration. It would be easy to give numerous instances where—if the reagents employed are less in proportion, or weaker than the required standard—we obtain not a smaller quantity of the product sought for, but a substance totally different, or, in certain cases, no action at all. Or if we, coming at once to the mark, fix our attention upon the higher animals, we find that for them all oxygen is the first necessary of life. If deprived of it for but a few minutes they inevitably perish. Yet Davy proved that animals placed in an atmosphere of pure oxygen are thrown into a morbid state, which, if the experiment is sufficiently prolonged, terminates fatally. Here, therefore, we see that dilution converts oxygen from a poison to a necessary of life. Or take sulphuric acid. If swallowed in a concentrated state it destroys all parts of the system which it touches. Dilute it largely with water, and it becomes a pleasant summer beverage of tonic properties. Open an animal which has swallowed such dilute acid, and examine the œsophagus and stomach with the most powerful microscope, and you will find no trace of those phenomena caused by the undilute acid. The results differ not in extent, not in degree, but in kind. These instances, which might be indefinitely multiplied, prove that the idea of poison is not essentially qualitative.

Let us now examine the second notion which has been brought forward to help us to a precise definition of poisons. It is alleged that whilst articles of food undergo decomposition in the stomach, poisons are not thus decomposed, but are absorbed unchanged, exist in the tissues unchanged, and in cases of recovery are finally expelled still in the same state in which they were first ingested. This explanation sins in the first place by implying that all articles taken into the system must be either food or poison. But, further, will chromic acid undergo no change if taken internally? How will the chlorides of arsenic or of cyanogen behave? There is again a liquid, neither rare nor entirely strange to chemists, which agrees with the features here ascribed to poisons as well as does alcohol. This liquid, if swallowed either in large or small doses, remains undecomposed in the stomach, is taken up by the absorbents, and enters the blood and the tissues, still without having undergone decomposition, and is finally eliminated as such in the urine and the perspiration. If, therefore, the above definition of poison be correct, this liquid is a poison; but if it is found by common experience harmless and necessary, what of the definition? The liquid in question is water, which has never been proved to suffer decomposition in the system, and which therefore *ought* to be a poison!

Leaving these discussions for more profitable considerations, we find included under the general name of "poisons" two classes of bodies having little in common save their injurious effects upon the living organism into which they are introduced. We have, on the one hand, certain ferments—"germs," or by whatever other name they may be known—which, when absorbed by an animal, bring on such diseases as rabies, small-pox, cholera, dysentery, typhoid, and the whole class of so-called zymotic affections. On the other hand, there are the true poisons, such as arsenic, strychnin, aconitin, hydrocyanic acid, &c. The substances of this latter class are well-defined chemical individuals, and even when, as is the case with all the most deadly, they are of vegetable or animal origin,—an awkward fact for the charlatans who guarantee the "purely vegetable" nature of their life-pills,—their efficacy has no connection with vitality. Poisons of this class when introduced into the system set up morbid action almost immediately, and if the dose be sufficient, and if no remedial measures are adopted, the symptoms go on increasing in violence without intermission until death ensues. A characteristic feature is that their noxious power may be decreased, or even totally extinguished, by dilution. If a pound of arsenic, or even of aconitin, were dissolved in the head-waters of the Thames, not the least danger would result to any of the thousands of people who drink Thames water as supplied to certain districts of London. The ferments, on the contrary, are not definite chemical principles, capable of being isolated, of entering into combination with other bodies, and of being separated again. So far as we know they are living organisms of low type, belonging to that "debatable land" that lies on the margins of the animal and the vegetable kingdoms, and they retain their virtues, or vices, only so long as their vitality endures. If swallowed, or otherwise introduced into the system, no evil effect is perceived for some time. There is a so-called "period of incubation" which in the case of *rabies* may extend over months, or even years, before the symptoms of disease make their appearance. Another characteristic is that the poison is multiplied in acting. If we kill an animal by the administration of a dose of strychnin or of arsenic, we cannot obtain from its body any more poisonous matter than we put in; but if we inoculate an animal with a ferment-poison we find in some cases its blood, in others its secretions and excretions, capable of reproducing the same disease in other animals. The ferment grows and fructifies in its victim, just as does wheat in a field. The ferments, too, lose none of their deadly power by dilution. If, returning to our former example, we placed a quantity of the excretions of a cholera patient or of a victim of typhoid fever in the head-waters of the Thames, there would be great danger to persons supplied with such water in London. By this dilution-test it has been established that the poison of the cobra—and in all probability of other venomous serpents—is not a ferment, but a true chemical principle. Hence all anti-zymotics—such as benzoic, salicylic, carbolic, and cresylic acids—are necessarily impotent in the treatment of snake-bites.

A distinction was formerly attempted to be drawn between poisons active wben swallowed and others supposed to be potent only when introduced directly into the blood. To this latter class the venomous secretions of snakes were supposed to belong. This is now known to be an error. The poison of the cobra, if in a sufficient dose, is as deadly when swallowed, or applied to a mucous membrane, as if conveyed into the blood. Some of the disease ferments are certainly active when swallowed, and, although there may be doubt concerning the poisons of rabies, of carbuncle, and of yellow fever, discretion is in all such cases the better part of valour.

The common sense of mankind has always deemed poison to be specific, like food, but in an even more decided manner. It has concluded from observation that a substance, harmful or even deadly to certain animal species, may be to others perfectly innocent. Certain modern writers, undertaking somewhat prematurely to explain the respective nature of foods and of poisons, have more or less explicitly denied this view. In the very teeth of experience they have asserted that any given substance is poisonous or otherwise in virtue of its own chemical attributes, no regard being had to the nature of the animal into whose system it is to be introduced. This opinion is no less contrary to general analogy than to direct observation. We bring together certain reagents in a glass vessel, and we obtain a given result; but if we perform the same experiment within the stomach or in the tissues of an animal, the result will in many cases be seriously modified. Nor can we even from an experiment performed upon one species conclude with certainty what will happen in the case of another. Nay, even within the limits of one and the same species we encounter in this respect more or less diversity. This is notably the case with the poisons of unwholesome mushrooms, and of certain species of fish and mollusks. It is no uncommon thing for a number of persons to partake jointly of such esculents, and while some of the party sicken, even fatally, the remainder are not in the slightest degree inconvenienced. Such idiosyncracies cling even to the life-less remains of animals. If different samples of wool are prepared in the same manner, and dyed together in the same pan, some of them will not only be darker in shade than others, but even a difference in tone will be occasionally detected.

Coming more directly to the point, we find certain animals feeding heartily upon plants which to other species, and especially to man, are decidedly poisonous. Snails and slugs devour the leaves of the foxglove as safely, and relatively in as large quantities, as a cow consumes grass, and they are also fond of the Solanaceæ, including the belladonna. The caterpillars of the following Lepidoptera feed upon poisonous plants :—*Gonopteryx rhamni* upon *Rhamnus catharticus; Thats polyxena* upon species of *Aristolochia; Danais Archippus* and *Chrysippus* upon various Asclepiads; *Deilephila Galii, Nicea,* and *Euphorbiæ* upon species of *Euphorbia; Chœrocampa Nerii* upon the oleander; *Sphinx ligustri* upon privet and laurel; *Noctua baja* upon belladonna; *Polia cappa* upon staves-acre; *Heliothisa armigera* upon tobacco; and *Chrysoptera moneta* upon monkshood. Turning to the Vertebrates we find that domestic fowls eat the foxglove; the hornbill consumes with perfect impunity the deadly fruit of species of *Strychnos;* whilst the hare and the rabbit nibble the belladonna and gnaw the bark of the mezereon.

We are by no means attempting to furnish a catalogue of species which feed upon plants noxious or fatal to man, but merely give a few of the best-established instances in proof that what is poisonous to one species may be to another perfectly harmless. There are doubtless poisons which are fatal to all. No animal save the serpent which secretes the poison has been found able to withstand the venom of the cobra.

In looking over the instances above given, and seeking their explanation, we may suppose that the deleterious principles of the plants mentioned may never be really absorbed. Like gum and cellulose, they may simply pass along the intestinal canal and be rejected with the solid excreta without ever entering the system at all. But this hypothesis, even if demonstrated, would fail to solve the question. We should still have to ask, why are these substances absorbed in one animal and in others harmlessly rejected? Further, in many cases we know that the poison is absorbed. When the common viper bites a hedgehog we know that the poison enters the blood. What renders it inert, as witnessed in the experiments of Dr. Lenz? Morphia if swallowed by certain apes enters their circulation, and can be readily detected in their urine in large quantities. Why is it in them eliminated without occasioning harm? We note, further, an important distinction: the exemption which any animal enjoys from the effects of some particular poison may be merely relative, as compared with the action of the same drug on man, or it may be absolute. To poison a hedgehog with cantharides would be as hopeful a task as to poison a Neapolitan with macaroni. But when it is loosely said that a horse cannot be poisoned with arsenic, or an ape with morphia, it is meant merely that these animals can endure larger doses than can we without perceptible harm.

There are cases in which copper—a well-known irritant poison—is not merely absorbed, but assimilated, and appears even to be an essential of life. Harless detected copper in the blood and in the liver of certain mollusks, especially cephalopods and Ascidiæ. His experiments show that it "stands in an essential relation to the blood-corpuscules." Indeed in the blood of the cephalopods there has been detected an organic copper compound, known as hæmocyanin, which fulfils the function due in the higher animals to hæmoglobin. Von Bibra detected copper on certain crustaceans, such as *Cancer pagurus*, and found that it occurred in an inverse ratio to the iron which it more or less completely replaced. Dr. Genth quantitatively determined the copper in the blood of the *Limulus*, a crab found on the coasts of North America. The blood of the healthy females, taken just before laying their eggs, was of a deep blue, and when dried the incinerated residue contained 0·295 per cent oxide of copper, and but a mere trace of oxide of iron. How strangely would the appearance of mankind be altered had we, like the *Limulus*, copper instead of iron in our blood! A rich azure would mantle on the cheek of beauty; we should extol in song the charms of sea-green tresses, and liken the lips we love not to coral, but to malachite. Perhaps the dealers in cupriferous pickles and preserved fruits and vegetables are striving to bring about this consummation.

Copper in an organic compound named touracin has been detected by Prof. Church in the feathers of the touraco. That it must consequently be normally present in the blood of this bird follows of necessity.

We may conceive it possible, both as regards disease-ferments and poisons properly so-called, that strains o beings indifferent to their action may gradually be formed by a process of natural selection. Scarcely any other hypothesis will explain the facts that certain races of men resist disease-poisons to which others succumb, and that an epidemic on its first visit to any country commits such frightful ravages. A striking instance of this was shown by the late spread of the measles in the Fiji Islands. It is exceedingly probable that the resistance of the slug to digitalin and atropin may have been produced in the same manner as the negro's non-susceptibility to yellow fever. Indeed one of the most curious facts connected with poisons is that individual men, and doubtless animals, by beginning with small doses, can gradually habituate themselves even to the most deadly compounds. The legend of Mithridates contains a basis of solid truth. Without depending upon disputed cases, such as that of the arsenic-eaters of Styria, the history of opium-eating gives abundant evidence of this acquired immunity. We at one time knew a gentleman, connected with the Inland Revenue in a town of the West Riding, who was no more affected by a wine-glassful of laudanum than is an ordinary man by the same quantity of sherry. Very similar is the case with tobacco, which, though it produces severe constitutional disturbance in persons unaccustomed to its use, is afterwards borne without the slightest inconvenience. The question may, indeed, be raised whether a similar immunity from snake-poison might not be acquired by habituating the system to doses at first infinitesimal, and gradually becoming larger and larger. Our knowledge concerning the physiological action of such poisons is as yet exceedingly rudimentary, and it is quite possible that there might be some serious objection to such inoculation. Still we should recommend this idea to the experimental notice of the medical profession in countries such as India and Australia, where venomous snakes abound, and where biological research has not yet been fettered at the bidding of a hysterical humanitarianism.

We mentioned above that on the entrance into the system of a true poison morbid phenomena at once make their appearance. To this rule there is an exception, *i.e.*, when the poison is administered in successive doses too small to produce the usual alarming symptoms, but too large or too frequent to allow the system to become habituated to their effects. Certain poisons indeed, such as lead, accumulate in the system, whilst others—colchicin being perhaps the best example—are cumulative in their action. We have thus a group of facts of which it is hard to speak without denying the facts we have just mentioned on the acquired toleration of poisons. We have, indeed, here to deal with very complicated effects. We know that the drunkard or the opiumist can swallow, without apparent result, doses of alcohol, of methyl- and amyl-compounds, or of laudanum, which would be rapidly fatal to a person "unseasoned." Still we know that this immunity is bought at the expense of his general health.

These considerations lead us to the question of "slow poisoning." Popular tradition and imaginative literature often introduce us to a very interesting villain, who gives his victims a dose calculated with such marvellous precision as—whilst producing no immediate action—to bring on inevitable death after the lapse of a certain number of months. Much of this may be safely pronounced impossible. Nevertheless a few ugly instances have occurred lately which warn us that in this department, as in many others, modern Science has not yet thrown an electric light into all the dark hiding-places of Nature. We know some of the details of the case of a retired Indian officer, who died not long ago at a town in one of the Eastern counties, and who displayed symptoms utterly irreconcilable with any recognised disease, and scarcely intelligible on any other hypothesis than that of an unknown poison administered previous to his return home.

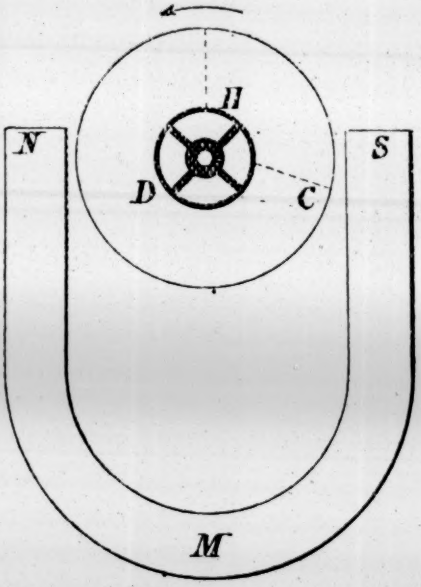
We have thus taken a hasty and necessarily superficial survey of a subject which presents to the chemist and the biologist numbers of unsolved problems—problems not merely of high scientific interest, but of the greatest practical importance. This tempting harvest-field is now, however, closed to Englishmen. Our laws, which with inimitable logic once deemed that "in a liberal construction copper is tin," now see "vivisection" where there is no "section" at all.

II. A CURIOUS THERMO-MAGNETIC MOTOR.

By Profs. EDWIN J. HOUSTON and ELIHU THOMSON.

DURING investigations by the authors, concerning the increase in the coercitive force of steel by changes of temperature, the following curious thermo-magnetic motor was devised. This motor, though devoid of practical value, will, no doubt, be of sufficient scientific interest to warrant a short description :—

In the figure, a disc or ring of thin steel, D, is mounted on an axis, so as to be quite free to move. The edges of the wheel are placed opposite the poles, H and S, of a magnet. In this position the wheel of course becomes magnetised by induction.



If, now, any section of the wheel, as H, be sufficiently heated, the disk will move in the direction shown by the arrow. The cause of this motion is as follows :—The section H, when heated, has its coercitive force thereby increased, and being less powerfully magnetised by the induction of the pole S than the portion C, immediately adjacent to it, the attraction exerted by the pole S on the latter portion is thereby sufficient to cause a movement of the disk in the direction shown by the arrow. If a constant source of heat be placed at H, a slow rotation in the direction shown is maintained.

To ensure success the disk must be sufficiently thin as to prevent its acquiring a uniform temperature. If the source of heat be at the same time applied at diametrically opposite portions of the disk, as at H and D, adjacent to the poles, the same effect will be produced. Since the amount of heat expended in producing motion of the disk is so enormous when compared with the force developed, it will be readily understood that this motor is of no value as such, but must be regarded as an interesting example of the interconvertibility of force.—*Journal of the Franklin Institute.*

III. THE HEAT OF THE COMSTOCK MINES.*

By Prof. JOHN A. CHURCH, E.M., Columbus, Ohio.

ONE of the most striking phenomena connected with the mines on the Comstock lode is the extreme heat encountered in the lower levels. This heat proceeds from the rock, which maintains constantly a temperature very much higher than the average of the atmosphere in Nevada.

The heat of these mines is a matter of more than usual interest, for they are the only hot ones now worked in the United States, and both in the present temperature encountered and in the increase which is to be expected as greater depths are reached, they appear to surpass any foreign mines of which we have a record.

The rock in the lower levels of the Comstock mines appears to have a pretty uniform temperature of 130° F.

The low conductivity of minerals to heat forbids the supposition that a rock of 130° F. temperature can lose heat sensibly to any depth in the course of twenty-four hours. The shallow holes which were made use of always lay in new ground, and exhibited results which may be accepted with as much confidence as if they were 20 feet or more deep. Very often they were in ground which had been exposed only one or two hours, having been sunk immediately after a blast which threw off 4 or 5 feet of the rock. The surface which was thus thrown down had not been exposed more than twenty-four hours. The high temperature and small flow of air in the heading forbid the supposition that any sensible diminution of heat could have taken place at the bottom of the drill-hole under such circumstances. The surface of the rock exposed to the air of the drift was found to be about 123° F., the experiment being made near the "header" or end of the drift. The air itself was found to show considerable uniformity when its temperature was taken under circumstances that were at all similar. In freshly opened ground it varied from 108° to 116° F., and higher temperatures are reported at various points, reaching in fact as high as 123° F. in the 1900 level of the Gould and Curry.

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* Abstract of a Paper read at the Chattanooga Meeting of the American Institute of Mining Engineers, May, 1878, by permission of Lieut. Geo. M. Wheeler, Corps of Engineers, U.S.A., in charge of U.S. Geographical and Geological Surveys, west of the 100th meridian.

The temperature of the air is subject to more fluctuations than that of the rock, for the simple reason that it is artificially supplied to the mine, and varies according to the distance to which it is carried, the quantity, velocity in the pipe, and its initial temperature. All of these elements of the problem vary within wide limits. The initial temperature of the air which supplies a particular drift will, for instance, depend upon whether it is drawn from the surface, the bottom of a shaft, where it is often cooler than above ground, or from some old air-way, where it has had time and opportunity to take up heat.

In the Comstock mines it is the custom, without exception, to blow the air through galvanised iron pipes, the diameter of which is usually from 8 to 20 inches. The size most used is 11 inches in diameter, and the usual amount of air blown is about 700 cubic feet per minute, this being the supply for two to six or more men, working in one or two "headers."

In most cases the air is not sent down from the surface, but taken from some point in the incline or at the bottom of the shaft. Its temperature may be assumed at about 80° or 90° F. in summer, though it is sometimes higher than this. Its velocity in the air-pipe is not very far from 1000 feet per minute. From these data it will be seen that about 15 or 20 degrees of heat are added to the air, in a period of time varying from half a minute to two minutes. The iron of the pipe is so thin and its conductivity so great that there is practically a slender current of air moving through a body of hotter air.

The iron receives heat both by immersion in the hot air and by direct radiation from the still hotter walls. The currents confined in it must be thrown against its sides by eddies, and the air is thus made to absorb heat by contact as well as by the transmission of heat-rays through it.

Drifts that do not exceed 200 or 300 feet in length are *usually* not above 110° or 112° F. in temperature, and more often they are below this. But when the length increases to 1200 and 1500 feet the temperature may rise to 116° F. without any other change in the circumstances.

So far as the author's personal experience goes, the latter temperature has not been exceeded in any drift into which a good current of air is blown. By a "good current" he means one of not less than 700 or 1000 cubic feet a minute. Still he has no hesitation in asserting his confidence in the higher temperatures which others have sometimes obtained. The view which he takes of the phenomenon and its cause admits of such exceptional heat at particular points as a rational consequence of the forces at work. But he regards them as exceptional, and believes the average temperature of those drifts which are considered to be distinctively "hot" is usually not above 108° to 112° F., though rising to 116° F., when they are very long.

These limits are, however, not in the least degree true of the water which enters the drifts from the country rock, and also from the lode rocks. That approaches more nearly 150° F. The large body of water which has filled the Savage and Hale and Norcross mines for more than a year, and from which it is safe to say a million tons of water have been pumped within twelve months, gave a temperature of 154° F. Even after being pumped to the surface through an iron pipe exposed, in the shaft of the Hale and Norcross, to a descending current of fresh air for more than 1000 feet, and then flowing for 100 or 200 feet through an open sluice in a drain-tunnel which discharges into a measuring-box, the water in this box was found to have a temperature of no less than 145° F.

But the water varies in temperature in different parts of the lode, like the rock and the air. In the East cross-cut 2000 feet level, of the Crown Point Mine, which is noted for its extreme heat, the water, after flowing for nearly 150 feet over the bottom of the drift, was found to have a temperature of 157° F. On the contrary, the water in other places is much less hot, but it is as a rule always hotter than the air, and in many cases it appears to be hotter than the rock is found to be, except in especially hot spots.

The East cross-cut of the Crown Point 2000 feet level, which was temporarily abandoned and boarded up on account of the heat, gave an *air* temperature of 150° F., the thermometer being thrust through a crack in the boarding. At the head of this cross-cut the heat was proved to be higher than this.

Another hot spot is in the Imperial Consolidated Mine. In this mine the Black Dyke splits, sending a shoot off to the north-east, and a drift has been run on the 2000 feet level, along the eastern side of this branch dyke.

This proved to be a very hot spot indeed. Rock, air, and water were all so much above the usual limits of temperature, even in these hot mines, that the work of cutting the drift must have been extremely severe.

The Belcher south incline has a hot belt of rock, quite narrow, a short distance above the nineteen hundred station, and similar hot places are found in most of the mines.

The author inclines to the opinion that, as a general rule, these hot areas lie in belts, and are not irregular or promiscuously placed in the mass of East country rock. Where this seems to be disproved by the distance run in the superheated rock, it will, he thinks, probably be found that the drift, or incline, and the hot belt have the same direction.

Hot belts are also found at the contact of the diorite and propylite in the Virginia mines. The diorite is itself in active decomposition, and mines which have carried drifts in or near it are very hot. The Julia has explored a quartz seam, which appears to lie entirely in the diorite, and this has proved to be one of the hot belts.

This apparent concentration of the heat in the line of contact of two rocks is not supposed to be due to any thermal or electro-thermal action, but to depend merely upon the fact that in this neighbourhood the ground is more broken and the surfaces of the rock increased. These conditions are obviously favourable to the action of atmospheric waters.

Belts of excessively hot ground are not the only noticeable phenomena in these mines. More remarkable still are the belts of unusually *cold* rock: these are fewer in number than the hot belts, but they are strongly marked; they are always wet, and the water that drips through the crevices of the shattered rock that composes them is noticeably cold to the touch, and cools down the air of the drift. Such a wet, cold belt of rock exists on the 800 feet level of the Justice Mine, and there is a very decided change of temperature in passing from one to the other side of it. Water drips from the rock in numerous places in these as in most mines, and that usually it is hot, or at least warm.

Other cold belts are found in the mines which are not so cool as that in the Justice, but are perceptibly cooler than the rock at a short distance from them. They complete a well-linked chain of heat phenomena, extending from rocks that are sensibly cold to the touch, and may not have a temperature above 50° or 60° F., through rocks that have the average atmospheric temperature, and those which are as hot as surface rocks ever become in Nevada, to those which have a temperature of 157° F.

Finally, in the chain of testimony relating to this phenomenon is to be noted the condition of the rock. Wet places have been spoken of, but the rock cannot be considered as generally wet. There are water-ways, and many of them appear to reach the surface, but they are of limited breadth, like the belts of hot rock. This water is usually hot, but sometimes cool or tepid.

Very often, usually in fact, the rock is perfectly dry, though very hot: that is the case in all the mines. Wet rock is the exception, and dry rock the rule, through the whole lode. In the drifts cut through this hot, dry rock, the walls of the freshly exposed surfaces are painful to the hand, and the air is often filled with dust. The rock is both hard and tough, but, in spite of its strength, it gives an impression of fine porosity to the touch, due probably to its trachytic character. It often has the odour of clay, but not always. It may be slightly adherent, or the impression of dryness upon the tongue may be due to its heat, or to the fine dust which covers every fragment.

The heat in the Comstock and other mines similarly situated is quite generally spoken of as the feeble remnant of a temperature that once reached the point of rock fusion, but the facts observed have led the author to refer the high temperatures encountered in the mines not to the internal heat of the earth, nor to the residual heat of the rocks, which were once melted, but to chemical action now maintained in the erupted rocks.

This action is not a combustion, for the oxidisable minerals in the lode and its accompanying rocks, the metallic sulphides, are little altered. In fact, the total quantity of pyrite and other sulphides is not large for the neighbourhood of a mineral lode, but on the contrary, strikingly small, and not sufficient to maintain the heat of the rocks and water, except under circumstances of unusually rapid oxidation. That no metallic oxidation of any moment goes on in these rocks is susceptible of proof. The metallic sulphurets in the rock show little sign of decomposition; and this is true even in layers of the propylite, that are fissured and seamy and drenched with water, whether hot or cold. In fact, the preservation of the sulphur compounds, in presence of so much heat and moisture, is a noticeable fact, which the author has frequently remarked in all the mines. The analyses of such of the mine waters as he has been able to find confirm this statement.

The author says the quantity of water pumped from the mines the past year must have been as much as 350,000 or 400,000 tons a month. If its temperature is assumed to be only 135° F., and the average temperature of the air for the year 50° F., we have in the year, say, $350,000 \times 12 = 4,200,000$ tons of water raised 85 degrees in temperature; or, as the usual expression is, $4,200,000 \times 85 = 357,000,000$ ton-heat units have been absorbed by the water. If the heating-power of anthracite coal is estimated at 7500 heat units to the ton, the heat in this water is as much as would be obtained from the combustion of 47,700 tons of coal. A cord of pine wood weighing 2700 pounds will probably give about 4300 heat units in practice, so that 84,000 cords would be necessary to keep up the heat withdrawn from the rocks in the mine waters alone.

If 10 tons of air pass through the mines collectively each minute, or 14,400 tons daily, and the air when discharged from the mines has an average temperature of 92° F., the total quantity of air for the year will be 5,256,000 tons, and the average rise in temperature 42 degrees. The specific heat of air being 0·267, we have—

$$5,256,000 \times 0{\cdot}267 \times 42 = 58,940,784 \text{ ton-heat units}$$

for the amount of heat absorbed by the air. This corresponds to an expenditure of 7859 tons of anthracite coal, or 13,707 cords of wood. The total quantity of heat carried out of the mines yearly by the water and air is therefore 416,000,000 ton-heat units, to produce which, in ordinary industrial operations, would require 55,560 tons of anthracite, or 97,700 cords of wood.

The number of men employed under ground in the mines of the upper Comstock is less than three thousand, and the heat from their bodies, together with that produced by the burning of the large numbers of candles, could not account for any considerable proportion of this heat. Indeed it may be assumed, in the absence of calculations, that all the heat from these and other ordinary sources of heat in mines is no more than sufficient to compensate for the large amount of refrigeration produced by the liberation of the compressed air which is employed in every mine to work numerous underground machines. This heat absorption has not been taken into account in the above calculations.

In another respect, also, these calculations are defective, and give results very much too low. Usually the air enters the mine dry and leaves it saturated with moisture, the evaporation of which indicates an amount of heat absorption, which would probably increase the above figures surprisingly.

These calculations, imperfect as they are, show that the source of heat is one that acts on a magnificent scale, and also that it cannot reside in the small quantity of pyrite which is oxidised. That source is probably the chemical alteration of the felspathic minerals of the propylite and other rocks. This change consists apparently in the process of transforming felspar to clay, technically known as kaolinisation, from the fact that china clay, or kaolin, is produced in this way.

With regard to the future increase of the heat it is judged that, until water temperatures above 154° F. are observed, nothing in the present condition of things indicates the certainty that the heat will ever rise to the boiling-point of water, 212° F. It is rational to suppose that the access of atmospheric air and water must diminish in proportion to the depth after a certain point is reached. At that point the temperature will be at a maximum. Below it there will be a state of equilibrium, probably for a very considerable depth. Below that the heat may diminish even to a point below that of the highest of the three zones. There must be some point where the absence of drainage allows the water to act like a blanket over the rocks, protecting them from the action of air or gases from the surface. The known depth required for the production of a temperature amounting to 130° F. is so great that we may fairly doubt whether air or water penetrate to lower depths in quantity sufficient to maintain mineral decomposition with the activity necessary to obtain the boiling temperature.

The author's explanation of the heat phenomena connected with these remarkable mines supposes the existence of a cold, and what may be called a burnt out, layer of rocks, extending for 1000 feet below the surface, a zone of hot rock still in active decomposition, which has been found to exist for a depth of about 1500 feet more, and no doubt extends thousands of feet further, and, finally, a mass of cold rock at a great depth, which has not yet begun to decompose.

The peculiar bands of hot and cold rocks which the author describes are simply layers of rock in which decomposition has been delayed or hastened. When the texture of a rock is such that it resists decomposition longer than other layers in its neighbourhood, it will be at its maximum temperature long after its fellows have passed theirs and cooled down, and this the author conceives to be the situation of the hot bands. They are individual layers of rock undergoing delayed decomposition.

On the other hand, when a rock is peculiarly susceptible to the action of the air and water, its alteration will proceed more actively than that of the surrounding rock. It will, therefore, pass its maximum temperature sooner, and be cooled down by the time that its neighbours begin to be at their hottest. This is the state of the cold bands. These bands, in fact, offer at several places in the mines examples in miniature of the action that is going on upon a grand scale throughout the whole system of rocks.

All the known facts strengthen the supposition which is advanced in this report, that the heat in the mines is subject to a steady and moderate increase as their depth is increased, this comparatively regular progression being broken by the passage through belts of rock heated above the average of the "country."

The author then considers the relation of temperature to depth, and concludes his paper by remarking that the Comstock mines offer a greater promise of discovery in this matter of rock temperature than any other he is acquainted with. The extraordinary rapidity with which their operations are prosecuted, the extent of the works, and the fact that they open to inspection a great eruptive mineral lode thoroughly for two miles in length, and partially for many thousand feet more, give them unusual value as a field for investigation. They not only follow an eruptive dyke throughout its course, but they also explore a parallel system of eruptive rocks by crosscuts, which are often from 300 to 500 feet long, and sometimes stretch out to 1000 feet and more.

They are also certain to be opened to much greater depths than now, and with a rapidity that will no doubt make them foremost in deep mining within a few years. These conditions, combined with the peculiar susceptibility of the country rock to decomposition, give good reason for expecting that they will before long be the scene of thorough and perhaps conclusive studies in this interesting subject of earth temperatures. This was but a secondary part of the author's work, which was chiefly confined to a geological study of the lode.

IV. MATTER ACTIVE.

"Tandem venitur ad formam substantialem qua adhuc abstracta per intellectum, remanet quoddam valde occultum quod est prima materia."—ALBERTUS MAGNUS.

JOHN DALTON.—You spoke to me lately of matter being dead. Death implies previous life, and in matter you will allow also that it implies capacity of living. We cannot after all expect matter to be alive of itself,—*i. e.*, isolated from the universe,—and is it not true that physicists have a general belief that when heat is gone the worlds cease? Human beings may be taken to illustrate this. If we consider any act—our walking, for example—it is only in part an act of our own; we rise up, and immediately gravitation draws us down; and if we look at dancers this mutual action between the earth and man is more apparent. The dancer seems to be glad because, for part of a second, he has overcome the attraction of the great earth; but he need not exult long, since down he must come, and the rhythmic struggle is a graceful tune of motion. It is a combined action of man and external nature, and similarly the elements are inert only when there is no sympathetic power to act with them.

Roger Bacon.—The analogy requires more than sympathy to complete it. The human being has a force within himself, and do we not find that analogous preparation in the molecules? The attraction of the earth is only a mean part of the work in dancing; the sun, too, if it does warm the elements and allow them to act, does no more; it gives no vitality that we know of, even when it removes that stiffness of cold which, as we before agreed, brought death to the matter which we see. Why, then, do people preach to us about the sun being the life of the earth and the source of all life? To those who say so I turn, with apparent contradiction, and say—No, the life is quite as much in the molecules. The sun gives probably several motions, but it cannot transfer its vitality to stones; the composite organism receives it, and disposes of it; the poor organism of the flint only becomes warm; the complex organism of the man glows with all his soul, the less complex molecules are moved in a still varied way. If they are of one original material, it may be asked, why does not heat set them in motion in exactly the same way? But we find them showing nearly seventy characters, *i.e.*, we have seventy bodies with fundamental differences to external appearance and tests. This is certainly not according to the development theory as we find it taken for granted in early writers, who uphold the existence of a *prima materia* or an undefinable *Yle*. Without heat it was agreed that there would be no action; with heat the elements develope many and peculiar characteristics. The first is simply that the motion which we call heat is transferred to them, an action of the same kind in all bodies whether solid or gaseous elements; by it iron and oxygen both have their particles more violently agitated. The next motion is different; it arises from a characteristic of the molecule—the oxygen and the iron unite. This shows a compound character in these substances; one for motion simple (let us suppose) in heat, another for combining. How many more we do not know, but certainly the second has very many modes of showing itself. If heat warms up the molecule its hidden qualities come out (just as we find when a starved man is set by the fire to warm), and all the various properties of the elements come forth more thoroughly than character from seventy people. In other words, the molecules have character; and is it not a general fact in Nature that character of any kind has some organisation to produce it? If we wish to give a compound mechanical nature to a bar of iron we make out of it several parts, and produce a more or less composite machine, and according to the work it does is the amount of organisation given to it. In animals there are found organs, material embodiments of character, and so in plants, although when we go down very low in size, or high in subtlety, we cannot see them. If a molecule can do several things, is it not probable that it is because of these several parts? Heat cannot make a simple piece of iron do the work of an engine; why, then, should we suppose a new law for a molecule, making a simple body do complex work?

John Dalton.—Allowing this argument to have some force, I cannot look on it as a final proof, and, even if it were, it is not any proof whatever that the original substance from which these atoms, as I call them, may have been made is the same for all; we may make our wheels of wood, iron, or brass.

Roger Bacon.—True; but all the organisations of plants and animals are made of the same materials, and it would appear that the organisations of the universe are also made of the same substances as the earth. If we seek a more simple substance, we may be satisfied that the same will do for all. There seems an analogy between your atom and a mechanism, and for my part I look on all the molecules of the elements as little heat engines; they do nothing until they are driven. This seems at least to apply to their chemical qualities. Interrupt a fly-wheel, and we have friction and heat; interrupt a molecule by some other molecule with which it combines, and we have heat also, and a stoppage of its ordinary motion; work in this case is done, and this work is the fabrication of a new product, a facture by the hands of Nature—a manufacture.

John Dalton.—I could agree to this facture, as you call it, in a sense; but are you not simply going back a stage to meet the difficulty as great as ever? If you make the usually received atoms of Newton to combine, do you not really do enough, giving them at their origin characteristics now found impressed on them by Nature?

Roger Bacon.—I imagined that an answer had been given to this: if we do so we make a great many compound bodies as the act of the first creation, and I might as well say why not suppose all the compound rocks to be made at once? why not allow the fossils to be the result of "a striving of inorganic nature after organic forms"? why do not all men rise ready grown or made from the red earth? This does not seem to be the will of Nature in any of the departments we have observed; it is unfair to suppose it in the small masses we speak of. This is an argument of probability, the compound character of your atom is an argument of fact, to my mind.

John Dalton.—You have quite forgotten one point. The atoms are not dependent on heat; they have the power of making it. They all gravitate, whether hot or cold, and if they were only allowed space they would rush together and produce heat in abundance, to give scope for all their chemical activities.

Roger Bacon.—We spoke lately of the very ingenious speculation of making supposed matter from ether; it agrees with the opinions of—let us say, for example—Albertus Magnus, who said, as the heading of this article denotes, that having taken away all the accidental forms we arrive at a substantial form, which being removed by the intellect there remains a something very occult, which is the first matter.

John Dalton.—I know something of the search for the *prima materia*, the something more than the elements, the fifth essence. Do you mean me to return to that mode of thinking from which, so happily as I think, Europe is freed?

Roger Bacon.—If our present atoms are compounds you must return so far as to seek for a substance out of which they were made, and I think that you are called on to do so intellectually even now, although I do not see a clear way of experimenting definitely. You have alluded to gravitation as presenting a mode of producing heat and setting all the phenomena of chemistry in motion, and certainly that power is wonderful. But if we were right before, then gravitation comes in after the atoms—molecules—are made. If the atoms were made from some other substance, we have no idea what that is, and cannot at any rate suppose it to gravitate; if it did we should find it accumulated surely; or if it could not be found by our usual observation it would be, to say the least, different from all our matter as we find it in the elements. We are therefore driven to a something which we cannot perceive, and which is not known to gravitate. If it does not gravitate it could not make these concussions to which you allude, and which produce heat. This matter, ether or otherwise, cannot therefore have made the suns by such means, and as a consequence the suns would not be the original source of the elements. This logic is not absolute, because we can suppose that some other power has brought the matter together; that, however, takes us another step back.

John Dalton.—You mean to say that you do not know anything to make atoms of, and you rush to a supposed something. I have them already made out of nothing at once.

Roger Bacon.—The elements all differ among themselves, of course; but some are like others, and show a brotherhood distinctly, as if the children of the same parents. Then the atomic relations distinctly point to parts made up in some manner, as distinctly as any of the qualities of compound bodies point to more than one part, as distinctly as potash and soda pointed to a decomposition in the eyes of Davy. We are therefore compelled to look for a something out of which they were made, and it is quite fair to suppose that body to be hydrogen, at least for the present, and that is a body with weight. My opinion, however, is that it is too like other elements to be the original one; it is too complex in character. I look to a simpler. I have called it *Yle*, or simply matter if you choose, matter abstractly with none of the known qualities of elements. These must be added in the school of creation. I should like a power to make the elements out of this Yle. You offer me the attraction of gravitation, but Yle does not gravitate. I have tried to get the sun to make them, and I confess it is a power so tremendous that it is a fair field for much speculation. There may be various stages of heat, and the atoms may become various at these stages, and there may be other powers besides heat, but so long as we cannot tell what will produce heat in ether without the finished atoms our speculation is imperfect.

John Dalton.—In any case you are driven back a step, and your difficulties are not diminished. You have yet found no power to make your little engines, your molecules, your atoms of the present. For my part I see nothing beyond, and am satisfied to begin my study of creation as late in its history as their facture.

Roger Bacon.—I know that I am driven back, and I recognise a time of creation when all was waste and void, when matter reflecting light did not exist, when atoms with chemical properties were not, when suns were not present to attract and planets for moving round were not formed, when dust itself did not show its presence in space, and life was of course far from appearing. I have arrived at this point at which so many have arrived before me, looking at the eternal ether, if not in the same way as Aristotle did, and looking at the primitive material as far more subtle than our grosser bodies, partly as the alchemists did, but by no means exactly so. I gave this idea in my works—that Yle produced all things, and that through it everything could be changed into everything. You will readily see that, if the elements are made of an antecedent, the step can be by the same power reversed, and matter may lose its existence and pass into ether. We can then suppose the sun to be a great agent for breaking the elements up into their primary parts, as well as we can suppose it making elements out of the *prima materia*. In whatever way we come to this *materia*, whether by beginning with it or returning to it, we are obliged to think of power—By what means did it produce atoms? The ether as it exists, be it ether or gas, conveys power; it therefore holds it in possession for a time; heat and magnetic influence from the sun are proofs. Some men tell us that power exists only in connection with our elements; this is not known to us; we judge of its amount and quality by its action on our elements, but we cannot suppose it arriving from the sun without passing the intermediate space. Power, therefore, exists of a kind very quick and subtle, and of a kind different from heat, and that power or these powers may be able to transform the elements, whether that itself be convertible into heat or not. There may be many powers made out of one original power, and this would seem better proved than that there are many elements made out of one original element. In any case their wondrous effects are partly seen, and the existence of these agents give us some clue to the very numerous phenomena which Nature produces—all of them, one might say, quite inexplicable. He that says that he understands one of them, let him explain. The object of this is to show that heat by concussion is not the original phenomenon; that heat also is not the original in all probability, but that elements must be formed before either exist. Something out of which they were formed, or are being formed, must have existed, or probably does exist, and power at any rate we must have to do anything, and that power is as mysterious as ever. If this is right reasoning, the history of the Universe has other stages than have been conceived; and as the body of man decomposes and composes, producing his life, so the Universe may be breaking up its matter and forming new matter at the same time. By this idea another glimpse is given us into the mode of comprehending the existence of time. We dare not say anything of the past and dread inconceivable eternity.

John Dalton.—When you get into that vague and poetical region I am obliged to assent, meaning that I believe that all is wonderful and mysterious; but have we made anything out of the discussion except this, that matter can do nothing without power, and that the power is not in the matter, such as we have it in our elements. If you mean so, I most gladly agree.

Roger Bacon.—We have made out that the power began not by forming elements, but an original something very subtle, and I repeat this to fix it on your mind, because I feel sure that you will soon give up supposing our atoms to be simple. This is an important conclusion, because it not only means that these little engines—atoms—cannot work without power, and they could not be made without power, so that, instead of being the actual powers that govern the world, they are in a double stage of helplessness, both requiring to be made and driven like other machines.

John Dalton.—You certainly gain a view farther back in creation, and have an idea of evolution of matter by this mode of thought, and no length of stages can change my knowledge that power must exist originally. Even those who tell us that heat, and therefore life, is got by gravitation merely reason in a circle; for what is gravitation? I am quite ready to believe that it is one of those characteristics that have been given to the elements, from which you are fairly entitled to call them compound; for even in my system I give a very compound nature without having thought of a compound structure to account for it.

Roger Bacon.—We must not stop here. The farther we go back the less does it appear to us that our matter has great power; it seems neither to make nor to unmake itself, and those who look to matter as beginning with excess of power and gradually dying out, only renewing itself partially by concussions until the final destruction, make a strange supposition, because the supposition involves a beginning of power made by matter, and a beginning of eternity, if we may so speak, as well as an end, so far as active matter is concerned. I see neither and can comprehend neither, and it seems to me that we are led in this reasoning back to power quite inconceivable developing matter: why this power did it so once and does not do it again there seems no reason for thinking, and it is possible to suppose this creative power as eternal more easily than it is to suppose the mere work which it performs to be eternal. If it worked before our mountains and our earth, it may work after them. It is just possible that the sun may prove a focus for Yle, to which to rush and be made into elements, and thus accounting for gravitation to the sun; but this would not account for mutual action. It is also possible to think that the sun may be using old material, making it new, and thereby lighting up creation with the rubbish. But whatever be the case our matter is not active enough to keep the Universe in motion of itself, and at best it may retain an impetus which may keep it going forward for a time, but of itself it is a slave; it works only when it is driven; and we are driven to conclude that neither from matter of our elements nor ether is obtained the power or forces that rule them.

We have been obliged to go farther back than the formation of our elements to seek the power, and lest the clue of the reasoning be lost I shall repeat the stages :—

The elements known to us will not act without heat.

They are therefore without life in themselves.

Heat is conveyed to them ; from the sun, for example.

There must be an agent of communication if heat be mere motion.

This agent we call ether,—let us spell it æther,—and to convey vibrations it must be material. (In excessive cold, communication of heat even from solid to solid can only be by means of æther.)

It would seem that æther does not gravitate ; gravitation is therefore not an essential of matter.

If we freeze down our elements to the most inert condition known, they still gravitate.

Therefore they are not the simplest matter ; they have a quality beyond that which æther has.

If made of anything we suppose the simplest thing we know of—we call that æther, but there may be a simpler still —let us say Yle.

Then as to priority. Heat made by the rushing together of the elements could not have been the original act of creation, because the elements, as we have them gravitating, require first to be made ; for we have seen that æther, or matter, is free from one of the most general qualities of our elements. It is simpler.

Heat therefore is not the great fundamental power of Creation.

Heat is a power to drive the elements when made.

We come, then, to an original, or at least an early, matter, less like matter than our own, and we seek a power to make elements from it.

Heat does not give life or activity to the elements ; it only produces one of the conditions in which they are able to act. Having various characters they must be variously organised, and heat sets them in motion accordingly.

Whenever we follow matter carefully we come to helplessness, immobility, death, unless revived from without. It is set in motion by a something which is manifestly separate from itself. But there are many intricacies in the thought, and we shall leave it for a while, if you please.

John Dalton.—There is nothing in your ideas opposed to mine. I was contented with that which I saw, but I must confess that you are more likely to be right than I was, in boldly making the atoms engines, as you call them, or compounds. I called them simple, and yet in reality I gave them so many properties that they became each a focus of powers instead of mere atoms, and I am willing to look farther back as you do for the beginning of things.

V. SOME NEW OPTICAL ILLUSIONS.*

By Silvanus P. Thompson, B.A., D.Sc.. F.R.A.S.,
Professor of Experimental Physics, University College, Bristol.

IN the Transactions of various learned bodies—the Royal Society, the British Association, &c.—papers have appeared from time to time describing various Optical Illusions. Some of these illusions have depended upon the duration of retinal impressions, some upon the formation of accidental subjective images, some upon the dispersion or irradiation of the eye, and some upon the phenomena of binocular vision. The illusions to be described in the present paper do not fall exclusively under any one of the heads enumerated, though they depend upon the duration of visual impressions, and upon a further and less perfectly understood property of the retina. They are all dependent upon *motion*, either of the object or of the observer, or of both. In each case that will be here brought forward there is a movement of the object across the field of view, and consequently of the image across some portion of the retina.

The most frequent illusions which arise thus are those in which one form of motion apparently takes some other form. As a most familiar instance of this kind of illusion we may take the case of the apparent motion of trees, hedgerows, and houses, as seen from a rapidly-running railway-train, the deception of the senses being most complete when the personal sense of motion is least.

When the train in which you are seated is drawn up beside another train, and then moves slowly forward, smoothly and without jolting, it is extremely difficult to tell whether your own train or the other one is in motion.

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* The greater part of this article was read before the British Association, at Plymouth, in September, 1877, an abstract only having as yet been published. A few additional facts were recently communicated to the British Naturalists' Society, and are embodied herewith.—S. P. T.

So when light clouds are drifted across the moon, one can frequently hardly resist the notion that it is the moon that is sailing along amongst fixed clouds; and if the drifting of the clouds be due to an upper current, while the lower air is still, the impression that the moon is sailing along past the clouds asserts itself with remarkable force.

I have observed an illusion closely akin to this at Clifton. Underneath the famous Suspension Bridge a zigzag path winds up to the top of the cliff, shaded overhead by trees. Walking up this path you see the bridge at intervals between the boughs, and, as the body rises and falls with the motion of each step, the bridge appears to be swaying violently up and down, as if it were blown about in the wind.

Many illusions akin to these very simple phenomena have been recorded from time to time. Three times—in 1845, 1848, and 1861—the late Sir David Brewster drew the attention of the British Association to some phenomena seen in railway travelling. If from the window of the carriage you look out at the pebbles and stones lying beside the line, you catch merely vague stripes, due to the rapid motion of their images across the retina; but on suddenly shutting the eyes "a motion is perceived in a direction transverse to the real impressions on the retina; and there is the appearance of lines complementary in the same transverse direction."* This Sir David subsequently referred to a subjective opposite motion going on simultaneously, and so causing a compensation of the impressions moving on the retina. In 1861 he returned to the observation, and compared the phenomenon with that obtained by watching the motion of a rotating disk with radial markings, directing the eye first to a point near the circumference, and then afterwards to a point near the centre, where the motion was slower. He concluded that there was a neutral line across the retina at which the compensation of the subjective impression was complete.

In the "Philosophical Magazine" for 1834 (p. 373) R. Addams described a peculiar optical phenomenon. After looking for some time at a waterfall, and then at "the sombre water-worn rocks immediately contiguous," he "saw the rocky surface as if in motion upwards with an apparent velocity equal to that of the descending water." This he ascribed to an unconscious recurrent movement of the muscles of the eye-ball, continuing after the gaze had been directed to the rocks, and thus occasioning a displacement of the images on the retina.*

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* Brit. Assoc. Report, 1845.

This illusion becomes more remarkable in the slightly varying case now to be mentioned. Watch the water of a rapid river, such as the Rhine immediately above Schaffhausen. The middle stream is running forward very rapidly. After watching it fixedly for some time, transfer your gaze to the slower stream near either bank. It actually seems to be running back.

I have also noticed, after watching a procession, that stationary objects appeared for a moment to be moving in a contrary direction.

In the "Journal of the Royal Institution" (vol. i., p. 609) an anonymous writer records a curious observation, that from a slowly-moving railway-train, while the stones and sleepers beside the line appear to fly back past the train, the neighbouring set of rails seems to be flying forward and keeping pace with the train. This he refers, and doubtless rightly, to the fact that the rails are of nearly uniform tint, and destitute of markings that would produce upon the retina impressions like those of the adjacent objects.

The railway affords many other instances of optical deception, and of these I will mention a few of which I am not aware that any specific notice has hitherto been taken.

When a landscape is observed from a moving railway-train, all distant objects from the near hedgerows to the distant hills appear to be moving past in the opposite direction, the nearer objects having the greater apparent velocity. Consequently, if the attention be fixed upon any object at some distance from the line, all objects beyond will relatively appear to be moving forward with the train, while objects nearer appear to be moving backwards. The combined effect is to make the entire landscape appear to be *revolving centrally* round whatever point we fix our attention upon.

Falling rain seen from a moving train always seems to fall obliquely (except in a *very* strong gale in the direction of the train's motion) in a direction opposite to that of the motion of the train. But if another train happen to pass in an opposite direction, and we look out at this and follow it with our eyes, rain-drops falling between the two trains will seem to be flying forward with ourselves.

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* An account of a very similar observation was communicated by Mr. J. Aitken to the Royal Society of Edinburgh, in November, 1878, apparently without any knowledge of the observations of Addams, Brewster, or of the author of this article.

If we stand on the platform of a station and watch a train approach, the end of the engine appears to enlarge or swell up as it approaches and occupies a larger area of the field of vision. Conversely the end of the last carriage of a retreating train appears to shrink down and contract as it diminishes in apparent magnitude. Stationary objects by the side of the line similarly appear to swell up as we approach them in a train, and to shrink together as we retreat from them. Curiously enough, this motion is also one which calls forth a certain "compensation" in the action of the retina. For, suppose we have been watching objects enlarging as we approached them, and then suddenly transfer our gaze to the side of the carriage opposite to us, we shall observe that it is apparently shrinking together and retreating from us. The opposite effect—that of apparent enlargement and approach—is produced as a subjective compensative action after watching objects from which we are retreating. The effect is more amusing if, after observing either of these cases of motion, we transfer our gaze to the face of a fellow-passenger sitting opposite.

An observer at some slight elevation above a railway, seeing two trains pass along the lines simultaneously in opposite directions, will receive the impression as of one long train moving round a circle. For when you look at a revolving wheel nearly edgewise, the nearer edge is seen moving past the farther edge, and in an opposite direction. The apparent motion of the two trains is the converse of this impression.

If from a similar situation two trains are observed, one moving rapidly, the other slowly in the same direction, the slower train may appear indeed to be moving in an opposite direction—a phenomenon similar to that of the Rhine above Schaffhausen already noticed.

Dr. F. Guthrie has noted the following illusion :—"Looking at the arms of a windmill in motion, in the twilight, or at such a distance that their attachment to the mill is obscure, we can, when the aspect is very oblique, easily imagine the arms to be turning in the opposite direction. We then fancy we are looking at the other side of the mill : so that if the sails are actually towards us in their descent, we fancy them away from us in their descent, which gives the notion of rotation in the opposite direction. This hallucination can, after a little practice, be as readily controlled by the will as can the introversion of a linear drawing representing a solid." *

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* GUTHRIE, Magnetism and Electricity, p. 243.

An analogous illusion is produced by illuminating certain vacuum-tubes with the sparks of induced electricity discharged alternately in opposite directions,* when the tube appears to be rotating about an axis perpendicular to its length and to the line of vision.

A crow flying along at dusk, seen against the sky at a low altitude, shows, when passing the observer, his wing above and beneath his body alternately. The effect of this alternation is as if he had but one wing, which seems to revolve round like the blade of a screw-propeller about its axis.

I have frequently stood upon the lofty suspension-bridge over the Avon, at Clifton, when large ships have been passing beneath. Under these conditions a curious illusion may be observed. If you look perpendicularly down on to a ship, as it emerges from beneath, it appears to be heeling forwards on to its bows; for as the masts emerge from under the bridge, and you see them growing longer as the foreshortening effect passes off, the mind cannot resist the notion that—like the windmill-sails—they are revolving round a centre. I have pointed out this effect to several persons, who have expressed much surprise at the completeness of the illusion.

The last set of illusions which will be described took their origin in an observation made by the writer early in 1876. He had been drawing a series of concentric circles in black and white, for the purpose of testing the astigmatic condition of the eye. Happening to shake the paper upon which the diagram was drawn, he observed a peculiar motion of apparent rotation of the circles. This illusion is extremely curious, and very easily reproduced. Let concentric circles in black and white be described upon a piece of card. If this be held firmly between the thumb and finger of the hand, and then a slight but rapid circular shaking motion be imparted by the wrist and elbow, the circles will appear to rotate upon the card. The hallucination succeeds best if the circles be clear and sharp at their edges, and the successive rings of black and white of equal widths. Their number and width is immaterial, but there seems to be a particular distance from the eye for each width of successive rings, at which the illusion succeeds best. Finely-drawn narrow rings must be held near, to produce a maximum effect; while to enable a number of persons to see the illusion at once the rings may be half or three-quarters of an inch in width, and to the number of fifteen or twenty.

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* See S. P. Thompson in Phil. Mag., 1876.

The radius of the circle of imparted motion should equal the width of a black or of a white ring, and the rapidity found most successful is that when each rotation occupies from one-sixth to one-fourth of a second. The rings appear to rotate once for every complete motion of the hand and card in the circular path, and in the same direction as the imparted motion. In this experiment each ring is displaced to a distance equal to its own breadth in every direction [illegible]cessively around its centre; and as the impression remains a short time on the retina, the optical effect is equivalent to that of a ring eccentric to an equal amount and actually rotating. Hence the illusion.

I have constructed a large number of patterns of curvilinear, circular, elliptical, eccentric, and concentric lines, many of which exhibit, in whole or in part, the same phenomena of apparent rotation. One of these is a single black circle, having a number of internal cog-teeth, upon a white ground. This circle, when shaken circularly in the manner described, appears to move round in the opposite direction to the imparted motion, and to move round through a distance of but one tooth for each successive complete motion. For circles possessing this property I have suggested the name of "Strobic Circles." Their motions are best seen when the eye is directed not exactly at the circles, but at some point near them. I have therefore found it more effective to have two strobic circles drawn side by side upon one card. That circle rotates most obviously on which the gaze is *not* fixed.

Further, I have noticed that if a strobic circle be "rotated," while a number of other circles are lying stationary within the field of view, when the eye was directed to the moving circle the others also began to "rotate."

This last observation cannot, I think, be explained on any supposition of unconscious muscular movement. In fact I entirely doubt the validity of this hypothesis in the case of Addams's observation upon the waterfall before cited.

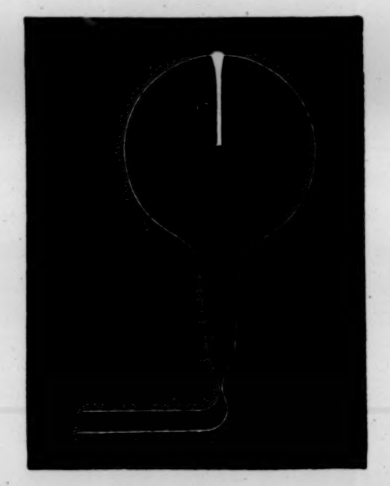
I am inclined rather to attribute these effects, and those of "compensation" in general, to waves of nervous disturbance moving over the retina; these waves, being of two orders,—one primary, and in the same direction as the objective motion of the images upon the retina; the other secondary and later in time,—giving rise to the subjective motions of compensation. I do not see how on any other supposition the phenomena noted in an earlier paragraph relative to compensative shrinking or expanding of objects can be explained. Such a hypothesis, will, I believe, also embrace all the other phenomena of apparent motion, except those which are the result of mental associations alone—such illusions, in fact, as those of the windmill and the flying crow. Such waves of nervous disturbance have, it would seem, a definite rate of propagation, probably not independent of the nature of the moving image with respect to colour, relative luminosity, and apparent magnitude. But whether these waves of sensible impression are due to a physical motion of any structures of the retina I am not yet prepared to offer an opinion.

VI. ON ELECTRICAL INSULATION IN HIGH VACUA.*

By William Crookes, F.R.S.

THE following experiments were suggested in the course of an investigation on the passage of an induction current through highly exhausted tubes. The main branch of the research being likely to occupy my attention for some time, I may be unable to return to these less important offshoots.

Fig. 1.



A pair of gold leaves were mounted, as for an electroscope, in a bulb blown from English lead glass tubing. The leaves were attached to a glass stem, and the lower part of the bulb was drawn out for sealing to a Sprengel pump, as shown at Fig. 1. A stick of ebonite excited by friction was generally used as the source of electricity, but any other source will do equally well, provided it is not too powerful.

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* A Paper read before the Royal Society, February 20th, 1879.

No special attention was paid to the action of electricity on the leaves in air or at moderate vacua, as it agreed with what is already well known. The exhaustion was pushed to a very high degree (about the millionth of an atmosphere), when it was found that the excited ebonite had a much greater effect on the gold leaves than at a lower exhaustion; for a long time, however, I was not able to charge the leaves permanently, in consequence of their falling together as soon as the source of electricity was removed.

When a hot substance was brought near the bulb facing a gold leaf, so as to warm the glass, molecular repulsion took place, and the leaves retreated from the warm spot, standing out at an angle of about 45°. As the glass cooled the leaves resumed their former vertical position.

While the leaves were repelled from the hot glass the excited ebonite had a very powerful action on them, and if it were brought near hastily the leaves flew off to the side of the glass, destroying the apparatus. By careful management and repeated trials, however, the ebonite could be brought near the warm spot of glass, the leaves suddenly extending at an angle to each other. The appearance was as if a spark had been able to pass across the bridge formed by the line of advancing and retreating molecules connecting the hot glass with the gold leaves. On the ebonite being removed and the glass allowed to cool, it was found that the repulsion of the leaves was permanent. The rubbed ebonite would attract and repel them as it was moved to and fro, but the angle formed by the leaves with one another remained unchanged. A warm body brought near the glass opposite one leaf would repel the pair as a whole; on then warming the opposite side of the glass repulsion on that side took place, the angle of the leaves being somewhat diminished, but on cooling the leaves opened again to their former extent.

When the glass bulb was strongly heated by a spirit flame the leaves suddenly discharged and fell together.

Another bulb (Fig. 2) was prepared, containing a plate of mica, *a*, which could be suddenly placed between the gold leaves, *b b*. The plate of mica was longer and wider than the gold leaves, and was connected with a small piece of iron wire, capable of moving up and down a tube sealed into the top of the bulb. By means of an outside magnet the mica plate could thus be lowered between the gold leaves or raised out of their way, as desired. The tube was exhausted to about the millionth of an atmosphere, the mica plate being held quite above the leaves. One side of the bulb was then heated, and the leaves permanently charged by means of the

FIG. 2.

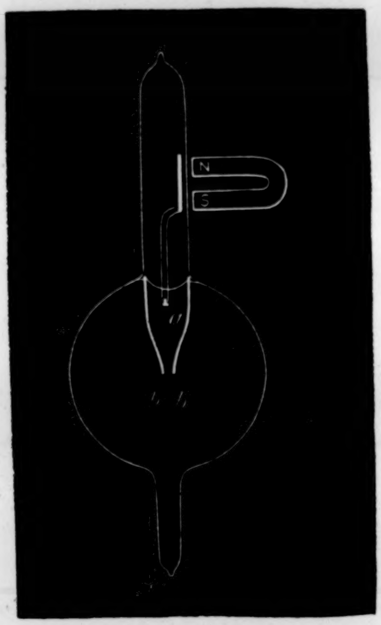
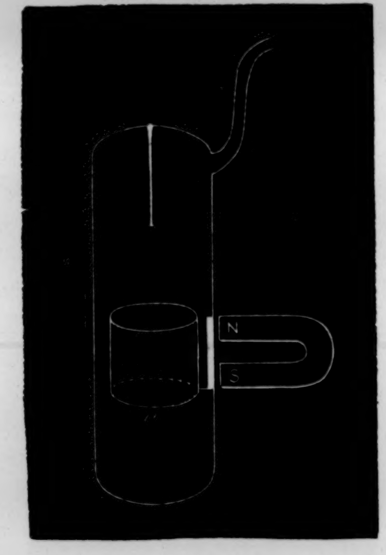


FIG. 3.

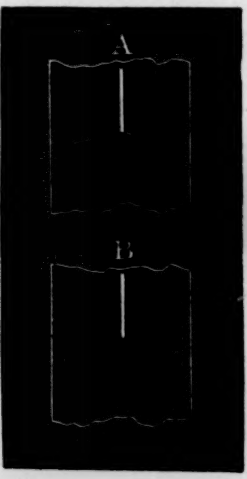


excited ebonite. The mica plate was now carefully lowered. As it came between the gold leaves they diverged farther apart, and kept so as long as the mica plate was between them. On removing the plate the leaves re-assumed their former divergence. This could be repeated any number of times.

A similar piece of apparatus (Fig. 3) was made, only instead of a mica plate coming between the leaves, a mica cylinder, *a*, capable of being raised and lowered outside the divergent leaves, was employed. I was not able to get entirely concordant results with this, owing to the friction of the mica developing electricity on the inner surface of the glass tube; but in all cases, when the cylinder was raised until it covered the electrified leaves, it had the effect of diminishing the angle which they formed with each other.

The following experiments were also tried:—The leaves being separated about 160°, as at Fig. 4, A, one side of the tube was slightly heated by a spirit-flame. The leaf on that side fell to a vertical position, and remained so when all was cold, the other leaf sticking out as before, as at B. This would seem to show that the divergence of the leaves in this case was not so much due to their mutual repulsion as to an attraction exerted on each of them by the inner surface of the glass tube. The remaining divergent leaf could be slightly lowered when the glass tube above it was warmed with a bunch of cotton-wool dipped in hot water. On cooling the leaf rose again to its original position. When this side of the tube was also heated with a lamp, the leaf was repelled down, but not so readily as the other had been, and

FIG. 4.



when the tube got cold it rose to nearly its former position. This was repeated several times with uniform results. When the leaf was repelled down the vertical leaf also moved away, so as to keep the same angle between them. It is therefore evident that the leaves themselves were also charged.

FIG. 4.

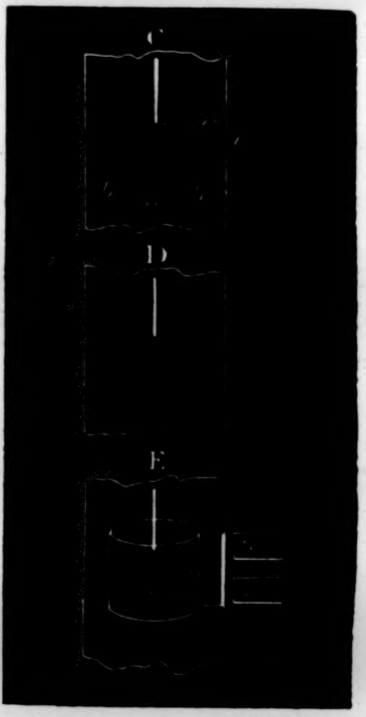


Fig. 4, C, shows the two positions of the leaves, *a a* before applying heat to the side *c* of the tube, and *b b* after heating the glass at *c*.

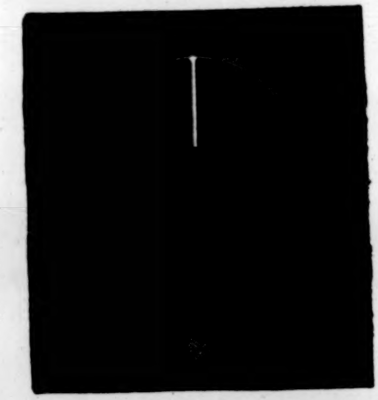
The tube was now heated on both sides, causing the leaves to come nearer together, as shown at Fig. 4, D. While the glass was warm the cylinder was raised so that it surrounded the leaves: this caused them to get a little closer together, and they kept in this position, shown at E, after the whole apparatus was quite cold.

After remaining thus for some time the cylinder was lowered, and the leaves widened out and took up the position shown at *b b*, Fig. 4, C. They did not return to the position *a a*, showing that their divergence was now owing to their own mutual repulsion, and not to an attraction of one or other to the electrified glass.

In December, 1877, I totally immersed one of these exhausted glass bulbs in a vessel of water, the gold leaves having previously been charged, and standing at an angle of 112° from one another, as at Fig. 5. The water was connected electrically with "earth," and the whole was set aside in a cabinet on the 1st of January, 1878.

At the present time, after having remained in this condition for thirteen months, the leaves form exactly the same angle with one another which they did when they were first put in the cabinet.

Fig. 5.



From this experience I think we may consider that at an exhaustion of a millionth of an atmosphere, air is an absolute non-conductor of statical electricity. It is therefore legitimate to conclude that the vacuum of interstellar space offers equal obstruction to the discharge of electrified bodies, without necessarily interfering with their mutual repulsion if similarly electrified. It is possible that in these facts an explanation may be found of some obscure celestial phenomena.

VII. SPIDER'S WEB FOR MICROMETERS.

THE web used is that of the common garden spider; the spinner of those geometric webs is abundant in summer and autumn. The reason why this spider is selected, rather than the house spider, may easily be seen by anybody who watches the proceedings of each when a fly is caught. The house spider seizes his victim and spins around him a winding-sheet of web, before carrying him into the larder. The garden spider binds around his captive a cord of web. It is this cord that is used for collimation.

In the autumn the mathematical instrument maker goes on his spider-hunting expeditions, generally on Sundays. He carries some pill-boxes in his pocket; selects well-fed, full-grown specimens, and puts each in a separate box, knowing the savage habits of his six-legged friends; for if two or more were put together in the same box, only a collection of amputated limbs and mangled bodies would be found on returning home,

The webs are secured for use and storage by making a fork of iron wire, 4 or 5 inches long, and 1½ to 2 inches between the bifurcations. The spider is held in the left hand and allowed to drop, which he readily does when dissatisfied with his quarters, but before falling he glues an end of cord to the finger, and then lets himself down easily by gradually spinning it out and hanging by it as it lengthens.

The instrument maker catches this cord across his fork, and, by turning, attaches it to one side; then he goes on turning the fork and advancing it, so that as the spider continues paying out his cable a series of obliquely crossing threads are wound upon the fork, which, when charged, is carefully laid in a box or drawer for use. The elasticity of the iron wire keeps the webs sufficiently stretched, and they are applied to the stop by simply laying the fork over it in such wise that one of the stretched webs shall fall upon the mark made on its face. When thus in position, a drop of varnish or glue, made by dissolving shellac in alcohol, is let fall upon each side; the spirit rapidly evaporates, and the web is fixed.

One of the odd results of this use of spiders is that many workmen become spider fanciers, and keep choice domesticated specimens that learn to spin their webs in convenient places above the work-benches, or in the bedrooms of their masters, who lovingly supply them with the fattest of blue-bottles. Though I forsook the trade on the expiration of my apprenticeship, I have not yet lost my affection for these animals—I never wilfully kill a spider. A sad story is told of the desolation of the late Mr. Troughton, who on one occasion engaged a new housemaid, and allowed her to commence professional operations without receiving the usual injunctions concerning his pets.—W. MATTIEU WILLIAMS, *in Journal of the Society of Arts.*

VIII. THE "JUMPERS," OR "JUMPING FRENCHMEN."

DURING the past year I have been investigating a manifestation of the Involuntary Life of the most novel and interesting character. It is found among the French Canadians, and is there known under the expression "Jumpers," or "Jumping Frenchmen." It appears, according to my researches, that a certain proportion of that people—mingled French and Indian blood—have acquired the permanent habit, which they cannot control, of jumping, or striking out with their hands, when commanded to do so, suddenly and authoritatively, by anyone who chances to be near them. The habit appears to have been acquired, in the first instance, by tickling one another, in the winter camps where they cut lumber in the Maine woods. They are a somewhat degraded race of beings; have few resources, very little intellect, and no mental discipline; cannot, usually, read or write, and, in their camps, while away the long winter evenings by playing upon each other's ticklishness until some of them get into a state of abnormal susceptibility that compels them to obey, automatically and instantly, any sudden order, as to strike, or to catch, or to jump, or even to vomit; they are at the mercy of their companions, and are frequently so much annoyed that they have to leave the camp where they are employed. The more they are played upon the worse they become; for the habit grows with exercise. This condition is not an epidemic, but a fixed and permanent state, and, so far forth, is different from the phenomena so often witnessed in revivals. It is, in fact, as I have elsewhere stated, a liability to be entranced on slight excitation; differing from the allied trance to which all of us are liable only in this, that it follows a very much milder irritation. When these "Jumpers" are excited to jump or strike, or to perform any of their peculiar automatic acts, they present the appearance of entranced individuals; their faces turn pale, their eyes are fixed and glassy, and sometimes their limbs tremble. One of these Jumpers is a waiter, and when told suddenly to "drop it," he at once drops whatever he may have in his hand, though it may be on the head of one of the guests, or on the floor. Another has so susceptible a stomach that he at once throws up his meals when any one but "gags" or makes the motions of vomiting in his presence; thus he has grown thin, and at one time was almost starved. One Jumper, when told to "strike," struck against a red-hot stove and burned himself. Accidents of this kind are quite frequent in their camps. One man, standing on the shore of a pond with a five-dollar gold piece in his hand, was told to "throw it;" he threw the money—a large sum for him—into the water. Another was standing near a kettle of fish; he was told to "jump," and he jumped into the kettle. When one of these Jumpers is addressed sharply and quickly in any language with which he is not familiar, he, at once and automatically, responds in that language. Thus, in numberless ways, they are abnormally susceptible to stimuli which, in the same degree, would have little or no effect on others.

In its relation to the subject of inebriety these extraordinary phenomena are of interest as illustrating the power and extent of the involuntary life, showing how varied and complex and subtle are the manifestations of this side of human physiology. These Jumpers, in the acts here referred to, are absolute automatons, utterly without volition or responsibility. Whatever responsibility there may be in these cases belongs to the time when the habit began to be formed—their first playing and trifling with themselves and others in the loneliness of their winter camp-life; they are no more to be blamed for their acts than are patients afflicted with St. Vitus's dance, or hysteria, or epilepsy, or with any form of insanity. The treatment, if any is used, should consist in removing the victims from the temptations of camp-life; they should be isolated, or, at least, kept away from those who are similarly afflicted, or who would take pleasure in playing upon, and thereby increasing, their weakness; at the same time, everything that educates and developes their higher cerebral centres will be of service. Indeed it has already been noticed that they grow worse by aggregation, and better by isolation. Their habit is a real and serious affliction to these people,—a source of anxiety and positive torment; they would rejoice to be delivered from it.

On the other side of the world, among the Malays, in the Island of Java, according to the "London Medical Record," phenomena precisely similar to those exhibited by the "Jumping Frenchmen" are seen. A woman carrying a child, and seeing one even pretend to drop any article, may at once drop that child. Many other interesting illustrations are given.

Very recently, also, my attention has been directed to some allied phenomena connected with a religious revival now in progress in a certain town in Vermont. The victims of this excitement roll on the floor or ground in most absurd and undignified attitudes; whence they are called "Holy Rollers." Unlike the Jumpers, however, these Rollers are not in a permanent liability to their disorder; when the excitement is over they will spontaneously recover. Just at present, in the height of the public enthusiasm, they are, on this subject, pure automatons. In the same line are the cases of starving girls, hysteria, and hysterical trance, like that of Mollie Fancher, of Brooklyn, which is now exciting so much enthusiasm. I have studied a number of similar and allied cases, and I never share the popular prejudice against them. They are without volition, practically irresponsible, and to be blamed—if blamed at all—for the *beginnings*, not the endings of their disease. Like inebriates, they are to be treated by taking them from their home and friends, and giving them a radically new environment.—G. M. BEARD, M.D., *of New York*.

NOTICES OF BOOKS.

Practical Physics; Molecular Physics and Sound. By FREDERICK GUTHRIE, F.R.S., Professor of Physics in the Royal School of Mines. (London Science Class-Books Series.) London: Longmans and Co. 1878.

FIVE-AND-TWENTY years ago physical laboratories were almost unknown in this country. At the present time all our large educational institutions possess a laboratory of this nature side by side with one devoted to chemistry. The fine physical laboratories at Oxford, Cambridge, Glasgow, and South Kensington may be taken as examples of what such places should be like, and of how they should be worked. At the latter some dozens of students are successfully put through a course of practical physics every year, under the guidance of Prof. Guthrie, who has given us some of the results of his experience in the handy little volume before us. An admirable manipulator himself, he knows well how to instruct others; and this work will not only be welcomed by the student, but also by the older man of science engaged in research, who will find many useful hints as to the preparation and use of apparatus.

Starting with some definitions and experiments relating to the cohesion of solids, the author passes on to the cohesion of liquids and gases, introducing us to some of his beautiful experiments on bubbles and drops. We object, however, entirely to the term "cohesion of gases," and we cannot at all understand the scope or object of the single paragraph (No. 19, p. 18) devoted to the subject, which ends with the sentence—"A more exact method is to place timed chronometers under bell jars containing various gases, and also *in vacuo*." The term "volume-elasticity," in the next Section, is awkward; the piezometer, on the same page, could only be constructed and worked by a very skilful manipulator, and when in action would scarcely repay the infinite labour expended upon it.

The subjects of Effusion, Diffusion, and Occlusion are ably discussed, and the most recent researches in each case are introduced. The short but comprehensive chapter on Specific Gravities abounds with practical hints, and details all the necessary processes. The ingenious way in which Prof. Guthrie has applied singing flames to the demonstration of the refraction of sound by gaseous lenses (pp. 81, 82) is deserving of notice; also the graphic representation (p. 102) of the cause of the clap, rattle, roll, and boom of thunder. The subject of Interference is ably and clearly treated.

The Appendix is not the least valuable part of the book: it contains a number of useful hints as to glass-working, fusing platinum wires into glass, and so on. Finally, a list of the apparatus and materials necessary for experiments in sound and waves concludes a volume which we cordially recommend to the notice of all students of physical science.

A Dictionary of Chemistry and the Allied Branches of other Sciences. By HENRY WATTS, F.R.S. Assisted by Eminent Contributors. Third Supplement, Part I. London: Longmans and Co. 1879.

THIS very valuable and standard work, which is without doubt the most exhaustive Dictionary of Chemistry in any language, furnishes, by the aid of its Supplements, a complete record of chemical research, within a short time of its appearance in the journals of scientific academies. Research is so abundantly on the increase that this closely-printed volume of 838 pages forms only the first half of the Third Supplement. The second half will appear before the end of this year, and will bring down the record of chemical discovery to the end of 1877, including, however, the more important discoveries of 1878.

Among the noticeable articles in Part I. we may mention Dr. Mills's account of Cumulative Resolution, Dr. Thorpe on Flame, and Mr. Warington on Barley and on Forest Trees. But the great feature of the volume is, without doubt, the very exhaustive article—presumably by the Editor—on the Benzenes and Benzoic Acids, extending over more than 150 pages. This includes an account of Körner's elaborate researches on the Orientation of the Benzene Derivatives, published in the "Gazzetta Chemica Italiana," and now for the first time made known to English readers. To this subject we venture specially to direct the attention of chemists, as one which is well worthy of their complete consideration.

The subject of "Cumulative Resolution" (a term proposed by Dr. Mills) has been developed by Wurtz, Watts, and Mills. The action is defined as "the combination of a substance, or mixture of substances, with itself *n* times, a particular portion of it being lost each time, according to some fixed law. Thus, bismuthic nitrate, when decomposed by a gradually increasing quantity of water, yields a series of bodies, which are less and less nitrogenous, and more and more bismuthic. Having regard to the denitration alone, we write—

$$n(Bi_2O_3.3N_2O_5)-(n-1)N_2O_5=Bi_{2n}O_{3n},N_{4n+2}O_{10n+5},$$

and by giving various values to n, from 0 to ∞, we shall obtain the formulæ of all possible compounds between $Bi_2O_3.3N_2O_5$ and

$Bi_2O_3.N_4O_{10}$." It is afterwards applied to various minerals derivable from two silicic hydrates, of which the second is the cumulate of the first.

The present volume goes down to the end of F: it embraces all new matters of any importance in Inorganic and Organic Chemistry, and in Mineralogy. We do not notice any articles on physical or technical subjects, and the great mass of matter relates to Organic Chemistry.

An Elementary Text-Book of Petrology. By FRANK RUTLEY, F.G.S. London: Longmans and Co.

THE study and recognition of rocks may fairly be considered as the alphabet of geology. without a knowledge of which the student may certainly become a reader, but never a worker in the science. In other words, he may peruse handbooks and manuals, may fix their contents in his mind, and pass an examination with credit, and yet be utterly unable to make an original trustworthy observation or to verify the theories which may be put forward. It is therefore strange that certain geological text-books—to particularise would be invidious—overlook this part of the subject altogether. The reader is supposed to be born with the power of recognising the respective species of rocks, and therefore at the end finds himself no more of a geologist than he was at the beginning. Mr. Rutley, in the text-book before us, makes a useful and praiseworthy attempt to supply the knowledge thus found wanting. Into chemical methods for the diagnosis of rocks and their constituent minerals he does not enter at length, the rather as all needful information on this head may be found in well-known and accessible works. He occupies himself the rather with physical, and especially optical, characteristics. The microscope—thanks to the exertions of the late David Forbes, of H. Witham, and especially of Mr. H. C. Sorby—is now no less essential to the geologist than to the student of animal and vegetable life, and our author explains clearly and fully the applications of this instrument in petrological research.

We can strongly recommend this work to all who are entering upon the study of geology, and who wish at the outset to lay a firm foundation.

The Realistic Assumptions of Modern Science Examined. By T. M. Herbert. London: Macmillan and Co.

We were under the impression that men of Science fully, though tacitly, recognise the necessity of a general postulate underlying all their researches and speculations; to wit, that phenomena are what they seem, and act as they seem to act. No one, to our best belief and knowledge, ever supposes himself capable of furnishing an absolute logical demonstration that the impressions of our senses are an invariably accurate copy of anything external. What everyone thus fully concedes Mr. Herbert sets himself to prove. In reply to the man of Science who taunts metaphysicians with the vanity and baselessness of their endeavours, he brings a counter-charge, which is merely a learned and elaborate version of the vulgar retort "You're another!" But that he adds any degree of certainty to human knowledge, or shows how we may escape from the difficulties he has so acutely pointed out, we are unable to perceive, and consequently we can only regret that he did not employ his great powers of mind in some other and more fruitful direction.

CORRESPONDENCE.

THE CHARACTER OF THE SEXES.

To the Editor of the Monthly Journal of Science.

Sir,—Mr. Murphy, in his "Habit and Intelligence," remarks that in man only do we find a reversal of the usual character of the sexes. "In all other species beauty is developed in the sex where the passions are strongest, and consequently generally in the male." Other writers, in discussing the origin of man, seem to take a somewhat similar view. But if we could ask the lower animals, would not each declare that its own species was the single exception to the rule that the male sex is the more beautiful? Civilised travellers, while fully recognising the beauty of women of the higher race, not unfrequently pronounce that among the lower savages the men are passable, but the women utterly repulsive.—I am, &c.,

S.

TRANSFORMATION OF SPECIES.

To the Editor of the Monthly Journal of Science.

Sir,—I see it mentioned among the "Biological Notes," in your last month's issue, that M. Bordier considers atmospheric pressure as one of the principal agents in the transformation of species. If variation is most rapid where pressure is most intense, should not the deep-sea fauna be richer and more varied than it has been found to be during the late *Challenger* Expedition? Are there any observations to show whether variation is greater or less at the sea-level than on table-lands and mountain-slopes?—I am, &c.,

J. W. S.

PROCEEDINGS OF SOCIETIES.

Royal Society, *January* 23.—A paper by C. William Siemens, D.C.L., F.R.S., "On certain means of Measuring and Regulating Electric Currents," was read. It is well known that when an electric current passes through a conductor, heat is generated, which, according to Joule, is proportionate in amount to the resistance of the conductor, and to the square of the current which passes through it in a unit of time. Dr. Siemens proposes to take advantage of this well-established law of electrodynamics, in order to limit and determine the amount of current passing through a circuit. The most essential part of the instrument employed for this purpose is a strip of copper, iron, or other metal, rolled extremely thin, through which the current to be regulated has to pass. One end of this thin strip of metal is attached to a screw, by which its tension can be regulated; it then passes upwards over an elevated insulated pulley, and down again to the end of a short lever, working on an axis, armed with a counterweight and with a lever, whose angular position will be materially affected by any small elongation of the strip that may take place from any cause. The apparatus further consists of a number of prisms of metal, supported by means of metallic springs, so regulated by movable weights as to insure the equidistant position of each prism from its neighbour. The current passing through the thin strip of metal passes through the lever, and the line of prisms to the terminal, without encountering any sensible resistance. A second and more circuitous route is, however, provided between the lever and the terminal, consisting of a series of comparatively thin coils of wire of German silver or other resisting metal, connecting the alternate ends of each two adjoining springs, the first and last spring being also connected to the lever and terminal respectively. Suppose that the current intended to be passed through the instrument is capable of maintaining the sensitive strip at a temperature of say 60° C., and that a sudden increase of current takes place in consequence either of an augmentation of the supply of electricity or of a change in the extraneous resistance to be overcome, the result will be an augmentation of temperature, which will continue until a new equilibrium between the heat supplied and that lost by radiation is effected. If the strip is made of metal of high conductivity, such as copper or silver, and is rolled down to a thickness not exceeding 0·05 millim., its capacity for heat is exceedingly small, and its surface being relatively very great, the new equilibrium between the supply of heat and its loss by radiation is effected almost instantaneously. But with the increase of temperature the position of the regulating lever is simultaneously affected, causing one or more contacts to be liberated, and as many additional resistance coils to be thrown into circuit: the result being that the temperature of the strip varies only between very narrow limits, and that the current itself is rendered very uniform, notwithstanding considerable variation in its force, or in the resistance of the lamp, or other extraneous resistance which it is intended to regulate. The resistance coils, by which adjoining contact springs are connected, may be readily changed, so as to suit particular cases; they are made by preference of naked wire, in order to expose the entire surface to the cooling action of the atmosphere. In dealing with feeble currents, Dr. Siemens uses another form of regulator, in which disks of carbon are substituted for the wire rheostat. The electrical resistance of carbon varies inversely with the pressure to which it is subjected. A steel wire of say 0·3 millim. diameter is drawn tight between the end of a bell-crank lever, and an adjusting screw, the pressure of the lever being resisted by a pile of carbon disks placed in a vertical glass tube. The current passing through the steel wire, through the bell-crank lever, and through the carbon disks, encounters the minimum resistance in the latter so long as the tension of the wire is at its maximnm; whereas the least increase in temperature of the steel wire by the passage of the current causes a decrease of pressure upon the pile of carbon disks, and an increase in their electrical resistance. The instrument first described may be adapted also for the measurement of powerful electric currents. The variable rheostat is in this case dispensed with, and the lever carries at its end a pencil pressing with its point upon a strip of paper drawn under it in a parallel direction with the lever by means of clockwork. A second fixed pencil draws a second or datum line upon the strip, so adjusted that the lines drawn by the two pencils coincide when no current is passing through the sensitive strip. The passage of a current through the strip immediately causes the pencil attached to the lever to move away from the datum line, and the distance between the two lines represents the temperature of the strip. This temperature depends, in the first place, upon the amount of current passing through the strip, and, in the second place, upon the loss of heat by radiation from the strip; which two quantities balance one another during any interval that the current remains constant. The thin sensitive conductor may be utilised either to restrict the amount of electricity flowing through a branch circuit, within certain narrow limits, or to produce a record of the amount of current passed through a circuit in any given time.

January 30.—"A Comparison of the Variations of the Diurnal Range of Magnetic Declination as recorded at the

Observatories of Kew and Trevandrum." By Balfour Stewart, F.R.S., Professor of Natural Philosophy in Owens College, Manchester, and Morisabro Hiraoka. In a previous paper by one of the authors a table is given exhibiting monthly means of the Kew diurnal declination-range, corresponding to forty-eight points in each year, or four for each month, that is to say, approximately one every week; and, in another paper, another similar table exhibits monthly means of the Trevandrum diurnal declination-range for weekly points. In the present paper these two tables are compared together. A comparison of the curves appears to lead to the following conclusions:—

(1.) Generally speaking, maximum points or risings in the one curve must be associated with maximum points or risings in the other, rather than with minimum points or depressions. Indeed, the researches of Broun and others, from a different point of view, strengthen this conclusion, which is, moreover, abundantly supported by a glance at the curves themselves;

(2.) The oscillations of the Trevandrum curve are greater than those of the Kew curve;

(3.) In many cases where there is a want of striking likeness between the oscillations of the two curves, there are yet noticeable traces in the one curve corresponding to the oscillations of the other. There are, however, a few cases where there is a want of apparent likeness.

(4.) In general, though not invariably, the oscillations of the Trevandrum curve follow rather than precede the corresponding oscillations of the Kew curve.

"On the Determination of the Rate of Vibration of Tuning Forks," by Herbert McLeod, F.C.S., and George Sydenham Clarke, Lieut. R.E. The paper commences with a description of the time-measurer adopted. The tuning-fork apparatus consists of a brass drum resting on friction wheels, and driven by a weight and train. Uniformity of motion being of great importance, an air-regulator, consisting of a fan enclosed in the lower compartment of a cylindrical box, is employed. Round one end of the drum are wrapped strips of paper on which white equidistant lines have been so ruled that they are parallel to the axis of the drum when the strips are in position. The strip most frequently used has 486 lines round the complete circumference of the drum. Opposite this graduated strip is placed a microscope with its axis horizontal. In the sub-stage is placed a 2" objective, producing an image of the graduations at the focus of the object-glass of the instrument. At the common focus of the two lenses is placed the tuning-fork, the stem of which is held vertical in a vice. If when the fork is in vibration the drum is made to rotate with such a velocity that one of the graduations passes over the interval between two adjacent graduations in the time of one vibration of the fork, a stationary wave is seen of length equal to the length of that interval. To determine the number of vibrations of the fork in a given time, it is only necessary, therefore, to be able to count the number of graduations which pass in that period. An electric counter gives the number of complete revolutions accomplished by the drum in any given period; and a fine-pointed tube, containing magenta, is carried by a saddle above the drum, and, being actuated by an electro-magnet, makes a dot on a piece of white paper wrapped round the drum at the beginning and end of the experiment. The distance apart of these dots gives the additional fraction of a revolution accomplished by the drum during the period of the experiment.

In illustration of the method a determination of one of Kœnig's forks was given:—

Duration of experiment, 5 minutes.
No. of 1st mark, 163. Number of line on circle, 301·0.
,, 2nd ,, 164. ,, ,, ,, 199·5.
Temperature by thermometer, 16·8°.
Number of revolutions shown by counter, 158.
Number of lines on circle, 486.

During the experiment the wave had risen in the field of the microscope one-half a wave-length above the hair.

$158\times486=76,788$ lines passed in 158 revolutions.
$301-199\cdot5=101\cdot5$ lines between two marks on drum.
$101\cdot5-0\cdot5=101$ lines corrected for movement of wave.
$76,788+101=76,889$ vibrations of fork in 5 minutes.
$\frac{76,889}{300}=256\cdot2966$ vibrations per second.

When this determination was made four other measurements were carried out, lasting for different intervals of time.

5 min.	... ...	256·297	at 16·8° C.
2 ,,	... ...	256·298	,, 16·9
3 ,,	... ...	256·293	,, 17·0
1 ,,	... ...	256·292	,, 17·0
4 ,,	... ...	256·292	,, 17·2
Means	... ...	256·294	16·98
Correction for error of therm.			−1·4
			15·58

Clock was losing about 0·8 second per day. Correction for clock rate—

$$256\cdot294\times\frac{86,399\cdot2}{86,400}=256\cdot292.$$

The maximum difference between the above numbers is 0·006, or 0·00234 per cent.

As frequent bowing might affect the rate, two sets of measurements were made, in one of which the fork was bowed every 20 seconds, and in the other every 5 seconds.

Duration of Experiment.	Bowed every 20 seconds.	Bowed every 5 seconds.
5 min....	512·470	512·460
3 „	512·460	512·452
3 „	512·460	512·456
Means ...	512·463	512·456

No change of phase has ever been observed on the application of the bow. The same result was obtained by Lissajous, although Poske has since found that bowing produces an alteration. The authors have never seen any sudden jump of the wave during bowing. To test this in another manner two forks were arranged, one horizontally and the other vertically, so that both could be seen simultaneously in the microscope (the horizontal one being beyond the sub-stage). The Lissajous figure is seen at the angle made by the two forks. When bowed no change of the form of the figure took place; its amplitude merely was increased. This arrangement of the forks is suggested as a method for comparing two forks of nearly the same pitch, or of a fork with its octave. A difference of amplitude produced no appreciable change in the rate of the fork.

	Large Amplitude. 0·37 m.m.	Small Amplitude. 0·15 m.m.
	256·277	256·275
	256·273	256·274
Means ...	256·275	256·2745

The mode of fixing the fork in a vice might have changed the rate: to test this two sets of measurements were made, in one of which the fork was fixed in the vice, and in the other on a sounding-box.

	On Sounding-Box.
	512·489
	512·482
	512·477
Mean ...	512·483

This result may be compared with that above given in which the fork was bowed every 20 seconds. It will be seen that the rate is rather less when the fork is held in the vice. In the previous paper the coefficient of change of rate for temperature is given as 0·00011 for each degree centigrade: this number has been fully confirmed by numerous measurements.

A set of Kœnig's forks have been measured with the following results, when corrected to 15° C. :—

A 256 fork measured in May gave	256·309	mean of	19 expts.
Same fork in June and October ...	256·310	,,	43 ,,
Another 256 fork	256·306	,,	12 ,,
A 320 fork	320·372	,,	18 ,,
A 384 fork	384·437	,,	24 ,,
A 512 fork	512·451	,,	30 ,,

These forks would be correct at a temperature of about 25° C.

An old fork of Sir William Thomson's, made by Marloye, gave 255·253 instead of 256. This is the mean of ten measurements. This fork would he correct at about 13° C.

Mr. McLeod informs us that the original suggestion of these experiments was made by his colleague, Lieut. Clarke, and that they carried out the experiments together.

PHYSICAL SOCIETY, *Annual Meeting, February* 8.—Prof. W. G. Adams, President, in the chair.

The following gentlemen were elected as Council and Officers for the ensuing year:—President—Prof. W. G. Adams. Vice-Presidents—Prof. G. C. Foster, Prof. R. B. Clifton, Lord Rayleigh, Dr. Spottiswoode, Sir W. Thomson. Secretaries—Prof. A. W. Reinold and Mr. W. Chandler Roberts. Treasurer—Dr. E. Atkinson. Demonstrator—Prof. F. Guthrie. Other Members of Council.—Capt. W. de W. Abney, Dr. Warren de la Rue, Major E. R. Festing, Prof. Fuller, Dr. Huggins, Prof. A. B. W. Kennedy, Prof. McLeod, the Earl of Rosse, Mr. G. Johnstone Stoney, Dr. Wormell. Honorary Members—Prof. G. R. Kirchoff, Dr. J. Plateau.

Dr. O. J. Lodge read a short paper on a method of calculating the curve of temperature in a rod along which heat is being conducted.

Mr. Shoolbred gave an account of electric lighting, illustrated by diagrams of the most recent magneto and dynamo-electric machines and examples of the lamps in vogue. The only surviving magneto-machine is that of De Meriten's, which is incomparably superior to the older ones of Nollet and Holmes. The dynamo-electric machines described were the continuous-current machines of Siemens, Gramme, Wallace-Farmer, and the alternating-current machines of Wilde, Gramme, and Lontin. Wilde's machine is the first of these, or parent machine, and Lontin's so resembles it that the latter cannot be used in England. In these machines the current from a continuous machine is passed through a second machine, which yields the alternating currents. In Lontin's machine, also, a number of distinct currents are generated in separate circuits, each of which is capable of feeding several lights. There is now one in use on the Western Railway of France which gives three distinct currents, each of which supplies four distinct lamps, making a total of twelve lights. The American Brush machine was also mentioned. The Dubosq lamp, which was the first regulator, is well adapted for laboratory purposes, but for practical purposes the Serrin is preferable. Rapieff's lamp is used in the *Times* office. The De Mersanne, which was highly spoken of at the Paris Exhibition, moves the carbons by bevelled gearings. The Wallace-Farmer lamp, though durable, is unsteady, perhaps because only inferior gas carbon has yet been used. Jablochkoff's candle was found to be defective from the solid insulator, such as plaster, used between the carbon. This made it very expensive also. Experiments in Paris had shown that whereas Jablochkoff's system cost 10d. per hour per light, the other systems cost only one half of that. In Wilde's candle the solid insulator was dispensed with, air taking its place, the arc always tending to keep at the top of the candle by electro-dynamic repulsion. In the De Meriten's candle three strips of carbon were used, the intermediate one being a stepping-stone to the arc which passes between the two outer ones. Werdermann's and Reynier's so-called incandescent lamps were also shown. Mr. Shoolbred, after alluding to the fact that the upper (positive) carbon takes a crater form, and hence becomes a reflector shedding the light downwards, stated that experiments had proved the line of maximum intensity of light to pass downward at an angle of 60° to the axis of the vertical carbons. By giving the positive carbon a horizontal displacement behind the lower negative one, Mr. Douglas, of the Trinity House, had been able to raise this line till it became horizontal, an advantage in lighthouses. He also pointed out that, whereas in Paris the Jablochkoff waxed for a period short compared to that in which it waned, in London it waxed for longer than it waned, which was of course an improvement; and Mr. Shoolbred suggested that it might be due to the fact that the engine worked at a speed nearer to that of the machine, and that the machine was founded more solidly in London than in Paris.

In the discussion which followed the reading of Mr. Shoolbred's paper,

Mr. Werdermann maintained that it was as easy to produce 500 lights as 10 from the electric light by sub-division, as he hoped soon to show, and stated that the size of the carbons greatly controlled the intensity of the light.

Prof. Ayrton held that the obstacle to the sub-division of the electric light was not an electrical one, but was due to the fact that the amount of light produced by the current is not in direct proportion to the amount of the heat produced.

Prof. Silvanus P. Thompson pointed out that residual magnetism in the cores of the bobbins of dynamo-electric machines lowered their efficiency, and hence short cores, as in the Wallace-Farmer machine, were an improvement.

NOTES.

BIOLOGY.

An anonymous writer in a contemporary, raising some interesting questions concerning the sting of the hive bee, declares the queen "stingless"—a novel doctrine.

The question of parthenogenesis among bees is still not absolutely decided, though the observations of Ziernon, confirmed by Sanson, decidedly support the affirmative view.

According to "Les Mondes" a young man died lately from having struck a match upon his finger-nail. A particle of phosphorus got under the nail, occasioning so virulent a burn that death ensued after twenty-seven hours.

It appears, from the "Royal Gazette" of British Guayana, that legislative measures have been taken to protect birds from the ravages of feather-hunters.

Prof. Belucci, in a carefully conducted series of experiments recorded in the "Gazzetta Chimica Italiana," has refuted Clermont's admission of the presence of peroxide of hydrogen in plants.

It is found that the human digestive organs are by no means able to extract all the nitrogenous compounds present in vegetable matters. Hence analytical results throw little light on their true nutritive value.

M. L. Couty has studied the physiological action of matê. He finds that its action is localised in the parts subservient to organic life, and especially the organs relatively least dependent on the nervous centres and specially on the brain. Upon the latter it has no apparent action.—*Comptes Rendus*, lxxxvii., p. 1091.

Dr. J. J. de Lanessan complains strongly of the neglected condition of French institutions for the study of the natural sciences. Thus at Dijon the Professor of Zoology is not even provided with a microscope.

M. Lacerda has laid before the Academy of Sciences certain results concerning the poison of serpents, as obtained from experiments on a *Crotalus* (species not named). He considers that the poison is a ferment, but not of the solid class, containing bodies resembling bacteria, and that it reproduces itself in the blood of animals which die from the bite of the serpent. As an antidote he recommends alcohol, both taken inwardly and injected under the skin. M. Quatrefages, in presenting the memoir, expressed serious doubts as to the author's determinations.

M. P. Geddes has presented to the Academy of Sciences a memoir on the function of chlorophyll in the green *Planariæ*. These animals, on exposure to sunshine, decompose carbonic acid, and give off bubbles of a gas which was found on analysis to contain from 45 to 55 per cent of oxygen, the residue being nitrogen. A chemical examination of their bodies, after extraction of the chlorophyll, demonstrated the presence of a considerable quantity of ordinary starch.

It is not generally known that there exists a vegetable organism. *Hygrocoris arsenicus*, which is developed in arsenical solutions, It appears as an opalescent cloud suspended in the liquid, and if examined under the microscope appears as a glassy mass scattered over with brilliant points.

M. A. Milne-Edwards has laid before the Academy of Sciences a description of *Blythonomus giganteus*, an isopod measuring 0·23 metre in length and 0·10 in breadth, and which is distinguished from all other crustaceans by the peculiar arrangement of its respiratory apparatus.

M. B. Renault has described a new group of silicified fossil stems of the carboniferous epoch. He establishes the existence of a series of types parallel to that afforded by the Sigillarineæ, but which in certain structural details approach the *Cordaites*.

In a memoir presented to the Academy of Sciences, M. J. de Seynes refers the "chestnut-disease" now raging in the Cevennes to a mycelium analogous to *Zasmidium cellare*, which he finds on all the roots of the trees.

The naturalist E. Beccari has discovered, in the virgin forests of Sumatra, a flower which surpasses all others at once in size, beauty, and perfume. It belongs to the family of the *Amorphophili*, and has received the name of *Titanum*. The diameter of the flower is as much as 83 centimetres. According to "La Lancette Belge" six chests filled with the roots of this plant have just arrived at Genoa.

Prof. Haeckel remarks that the intellectual contrasts existing between the ants and their cattle, the Aphides, are certainly greater than the enormous difference which we recognise between the divine genius of a Goethe or a Shakspeare and the poor animal soul of an Australian negro.

According to Dr. Polli the human organism undergoes, in the course of its existence, a slow oxidation, on the completion of which death ensues. According to his calculation this should happen, accidents excepted, not earlier than the hundredth year. To prolong life a few grammes of a sulphite should be taken every morning in a glass of pure water.

"La Lancette Belge" calls attention to the existence of numerous poisonous fishes on the African coasts, which it fears may be brought into European markets preserved by the application of artificial cold.

According to Prof. Haeckel no expression has given rise to so many errors and misunderstandings as the word "instinct."

PHYSICS.

M. Sergius Kern sends us an account of a very interesting experiment by M. Slouginoff, which is likely to be fruitful in physical research. He takes the two electrodes of a battery, one of which is a thin, plain platinum plate, placed horizontally, and the other is a platinum wire, placed perpendicularly to the plate and nearly in contact with it. A small quantity of water, acidulated with sulphuric acid, is next poured on to the plate. If a current of 8 to 12 elements passes through the apparatus, and the wire is made the cathode, a spot of light is observed on its point. When using 15 elements the light appears, even if the direction of the current is changed. During these phenomena the water is only slightly decomposed, and the needle of a galvanometer, if introduced into the circuit, is only slightly deflected. It was also remarked that the surface of the water under the wire is lowered, forming the shape of a cup. The bubbles of gas arising from the decomposition of the water, in this case, travel constantly round the wire, forming a very pretty figure of 8. This is caused by the movements of the surface of the water, it being alternately repelled or attracted to the wire carrying the current. The gaseous bubbles obey these movements. It was further discovered that during these actions in the apparatus the current was intermittent. It is well known that the mechanical movements in iron or steel bars, during their magnetising by discontinuous currents, may be used as a source of sound. In employing a current of 12 elements, and introducing into it Slouginoff's apparatus, a distinct sound is remarked. If the platinum plate is well polished the water is repelled from the point of the wire equally in all directions and some millimetres from it. The current is thereby interrupted, and the liquid advances to the wire; in this case the liquor will be again repulsed, and so on. Taking advantage of these actions of the current, the apparatus may be employed as a very simple form of an electrical interrupter.

At the Technical Society of St. Petersburg M. Latchinoff recently delivered a lecture on the electric light. He made some experiments with Jablochkoff's condensators, which consist of a set of tin plates placed one on another; the surface of every plate is 0·7 square metre. Between every pair of such plates a piece of silk covered with varnish is introduced. The height of the condensator is about 6 feet. On introducing two condensators into a circuit the intensity of the electric light is doubled.

The lecturer believes the new system of electric lighting devised by M. Rapieff to be a serious opponent of Jablochkoff's process. The chief advantage of the new system is that the luminating point does not change its position, and therefore this system is more suitable for the projection of the electric light at a distance. This advantage will give increase to the use of the electric light for military purposes.

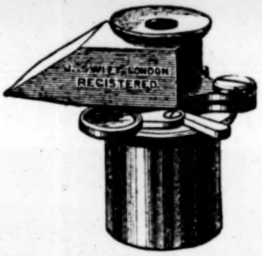
At a recent meeting of the French Academy of Sciences a paper by M. Henri Becquerel, on the "Temporary Magnetic Proportions developed by Induction in certain Specimens of Nickel and Cobalt," was read. The specimens of iron used by M. Becquerel were taken from the same piece of Swedish soft bar iron, of a high degree of purity. By submitting bars of iron and nickel, of the same dimensions and shape, to gradually increasing magnetic influence, it was found that the ratio between the coercitive force acquired by the iron and the nickel is a quantity varying with the intensity of the magnetic force used. This ratio begins by being a minimum of about 0·4; it then increases to 0·75, and again decreases to a still lower minimum of about 0·2. A nickel bar becomes saturated much more quickly than a similar iron bar; the magnetism of the former remains stationary, while that of the latter goes on increasing. Similar results were obtained with a bar of nickel, which was allowed to oscillate over the poles of a powerful magnet. The experiments with the bars of cobalt were similar in their results.

At the same meeting M. Héraud sent in a paper "On a New Voltaic Pile," in which the exciting liquid is a saturated solution of sal-ammoniac, to which one-tenth of liquor ammonia has been added, the depolarising substance being mercuric proto-chloride. With a closed circuit the ammonic chloride is decomposed, the chlorine going to the zinc and the ammonia splitting into ammonia and hydrogen, which reduces the mercury salt to the metallic state, ammonic chloride being once more formed. Each cell is hermetically sealed. After being in action for 227 days, a pile of 9 elements gave an electromotive force equal to 0·73 per cent of the original strength, and at the end of 984 days 0·50 per cent.

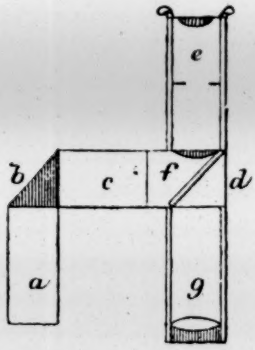
Four kinds of camera lucida were brought before the Royal Microscopical Society at their December meeting. Dr. Hoffmann's instrument takes the place of the usual eyepiece. The image of the object is viewed by two reflections; the first by a plate of silvered glass; the second by a plate of transparent glass, through which the paper and pencil are viewed directly. The instrument has the disadvantage of interfering with the employment of the ordinary eyepiece, which has to be removed before the camera can be used, and only permits drawings to be made with the microscope in either vertical or horizontal positions.

A somewhat complex arrangement of prisms for the same purpose, by M. Pellerin, was also described, but no drawing is given, and indeed the author's original paper is without illustrations.

Mr. Swift's apparatus is a modification of a well-known form by Nachet. The object is viewed in the usual way, through the eyepiece; the projecting box attached to the cap contains a prism, which reflects the image of the pencil and paper to an inclined tinted plate, where a second reflection directs the image upwards and renders it visible with the object. This instrument has the advantage of permitting the microscope to be used in any position. A second disc of neutral-tint glass can be interposed when the light from the object requires to be still more subdued to render the pencil point visible.

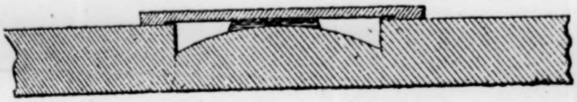


In Dr. Russell's instrument a tube (*a*) containing a right-angle prism (*b*) fits into the microscope. The image is reflected to a tinted plate (*f*), which again reflects it through the eyepiece (*e*). The tube (*d*) is prolonged downwards, and carries a telescopic object-glass (*g*), which forms an image of the paper and pencil combined with that from the microscope. The image of the paper requires inversion, either by a reflecting prism (*g*) or suitable lenses placed below the object-glass.



Dr. Hudson, in the discussion which followed the reading of the paper, considered ruled squares placed in the focus of the eyepiece, the contents of which were copied on paper with similar ruling, superior in simplicity and ease of working to any form of camera lucida, especially in the rapid drawing required when living objects were examined.

Mr. Weber, by a simple variation of the hollow slide, has greatly increased its utility. The cavity, instead of being concave as usual, is ground out, as shown in the cut, having the greatest depth at the edges, the bottom of the cell being convex.



A drop of water is held by capillary attraction between ths cover and the projection; the cover may be fastened or not, at pleasure.

A writer to "Science Gossip" describes a very simple live box, constructed of two ordinary 3×1 slides, placed between two easily fitting flattened brass bands. The slides are kept apart by an india-rubber ring of any convenient thickness, and the whole contrivance kept tight by means of wedges. The contrivance is readily constructed, and applicable to many purposes where a thin glass in front is not required.

The inhabitants of Heilbronn, the native town of the late Dr. Julius Robert Mayer, so renowned for his contributions to the Mechanical Theory of Heat, have resolved to erect a suitable memorial on the spot where he lived, laboured, and died. The following gentlemen have agreed to form an English Mayer-Memorial Committee:—William Spottiswoode, Esq., Pres. R.S.; Sir Joseph Hooker, K.C.S.I., V.-P.R.S.; Prof. Stokes, Sec. R.S.; Prof. Huxley, Sec. R.S.; Prof. Tyndall, F.R.S.; Dr. William Siemens, F.R.S.; Herbert Spencer, Esq.; Sir William Gull, Bart., F.R.S.; Sir James Paget, Bart., F.R.S.; Sir John Lubbock, Bart., F.R.S.; Prof. Henry Smith, F.R.S.; Dr. Debus, F.R.S.; George Busk, Esq., F.R.S.; Dr. Hirst, F.R.S.; Prof. Frankland, F.R.S.; Prof. Dewar, F.R.S.; John F. Moulton, Esq., M.A.; Frederick Pollock, Esq., M.A. Subscriptions exceeding one pound may be sent by cheque to Messrs. Robarts, Lubbock, and Co., 15, Lombard Street. Smaller sums may be sent by post-office order to the Honorary Secretary, T. Archer Hirst, Royal Naval College, Greenwich.

CHEMISTRY AND TECHNOLOGY.

The substance of which the much-talked-of Swiss "self-luminous clock-dials" are composed has, according to "The Engineering and Mining Journal," been examined by Prof. Henry Morton, of the Stevens Institute of Technology, Hoboken, and has been ascertained to be the "sulphide of calcium," which is caused to adhere to the dial surface by means of some resinous varnish. This compound has long been known to possess phosphorescent qualities of a high order; but as prepared for this special use its phosphorescence has never before been equalled, as may be judged from the fact that, after being shut up in a box for five days, it was still visible in total darkness. Prof. Morton expresses the opinion that further advances in this direction may develop discoveries of most surprising character.

We gather from the same journal that Mathey, a Neufchatel chemist, communicates the following facts relating to the same subject. The phosphorescent dials, he states, are usually made of paper or thin card-board, enamelled and covered with an adhesive varnish, upon which is dusted, with a fine sieve, powdered sulphide of barium. The sulphides of strontium and calcium possess the same property; but our authority affirms they lose their phosphorescence more quickly than the barium salt. When the dial has lost its self-luminous qualities they may be restored by an hour's exposure to sunlight, or by burning near it a few inches of magnesium wire.

In a paper presented to the Academy of Sciences, on the "Classification of Colours and the Means of Re-producing Coloured Appearances by Three Special Photographic Proofs," M. C. Gros distinguishes two categories comprised under the word "colours"—lights and pigments. The elementary lighis which by their mixture produce all kinds of shades are the green, violet, and orange rays. The elementary pigments which by their mixture produce all kinds of shades are red, yellow, and blue. To obtain directly the elementary tints of rays and of pigments it is sufficient to look through a prism at a white stripe upon a black ground, and at a black stripe upon a white ground. In the first case an orange, green, and violet spectrum is seen; and in the latter case a blue, red, and yellow spectrum. In the former case the orange, green, and violet are elementary rays; and in the latter the red, blue, and yellow are rays combined two and two. The author describes an apparatus which he names the chromometer, and by means of which he produces the photographic effect above mentioned.

A daily paper, speaking of the prizes at the Paris Lottery, uses "carbide of natrium" as a synonym for carbonate of soda. Another newspaper paragraph describes the death of a man who fell into a cistern of "caustic and potash."

According to the "Revue Britannique" ether is consumed as an intoxicant by ladies of rank in England, the grass in Hyde Park being strewed with empty bottles flung from carriage windows.

According to M. Galippe, human hair cut during life has a special odour which remains after the action of potassa or of any other reagent. Hairs which fall off naturally are inodorous, dull, not silky to the touch, and present a special phenomenon of alternating colouration. The hairs of the Chinese, even after the action of potassa, possess an odour of musk, which is intensified at higher temperatures; their section is not round or oval, but polyhedric, and though black they appear red by transmitted light. The hairs of certain persons retain a peculiar electric condition: if the hand is approached to a bunch which contains some hairs of this kind, they will separate themselves from the remainder and follow the hand.

A curious toxicological case is reported from Hamburg in the "Chemiker Zeitung." The body of a man who died in 1867 was taken up for examination. It was thought necessary to determine arsenic not merely in the corpse in question, but in the soil of the churchyard at different distances from the coffin, and also in the body of another man who had been subsequently buried in the same grave. This latter body was perfectly free from arsenic, which, however, was found in the first corpse in amply fatal quantity (0·24 grm.), whilst in the lid of the coffin and in the adjacent soil very minute quantities were traced. Hence the conclusion was fairly drawn that the man in question had been poisoned with arsenic, and that a portion of the poison had been gradually transferred from his body to the wood of the coffin and the adjacent soil.

In a paper on Poisoning by Mushrooms, in the "Moniteur Scientifique," Mr. J. A. Palmer says that mushrooms may act as a poison in three different manners. They may act as an indigestible matter, which is the case with hard coriaceous species, and may even occur with the edible mushroom when decomposing, as it gives off sulphuretted hydrogen in quantity sufficient to cause vomiting. Or, again, they may be gelatinous or acrid. Many *Boleti*, otherwise innocent, are too gluey to serve as food. Lastly, mushrooms may contain a subtle alkaloid, devoid of smell and taste, as happens in the group of the Amanitæ. This compound is known by the name of Amanitin, and to it the fatal cases of mushroom-poisoning are mostly due. No remedy has yet been found. No immediate effects are produced by this poison; but after eight to fifteen hours the patient experiences stupefaction, nausea, and diarrhœa, followed by delirium and death. Mushrooms containing this poison seem able to communicate it to wholesome species by contact, and it may also be absorbed through the skin. The author was on one occasion seized with alarming symptoms after carrying in his hand some Amanitæ wrapped in paper.

ENGINEERING, METALLURGY, MINERALOGY, AND MINING.

According to the "Colonies and India" a valuable deposit of graphite, of excellent quality, has recently been made in the province of Wellington, New Zealand.

M. F. Pisani has communicated to the Academy of Sciences the fact that Wagnerite—a fluoriferous phosphate of magnesia, originally met with at Werfen, in Salzburg—is identical with a mineral subsequently discovered at Bamle, in Norway, and provisionally named Kjerulfine. The latter, however, contains a percentage of lime. M. F. Pisani also finds that the Russian retinite was formerly mistaken for a manganiferous garnet.

A new meteoric mineral, named Daubréelite, has been recently described by Prof. J. Lawrence Smith. In a state of purity it consists of shining black fragments of more or less foliaceous structure, somewhat resembling molybdenite. It is not in the slightest degree attacked by hydrochloric acid, cold or hot, but dissolves slowly in warm nitric acid, without depositing sulphur. Its specific gravity = 5·01; its composition being—sulphur, 42·69; chromium, 35·91; iron, 20·10.

The fifth Report of Progress by the Secretary for Mines of the Geological Survey of Victoria discloses no very salient facts, either in the department of economic geology or of palæontology. An examination has been made of certain elevated basaltic plateaux known as the Bogong and Dargo High Plains, situate on the eastern boundary of the colony—a well-watered district, but from its great elevation subject to frosts even in summer. The region is auriferous, but the distribution of the precious metal is very unequal. Mr. Krause's Report of the Wombat Hill District shows that the auriferous leads are very rich. From 1 mile of this lead 170,000 ozs. of gold have been obtained, equal to a yield of 32 ozs. of gold per lineal foot. In the deep quartz mines of Stawell there is no falling off in gold on descending lower—a fact of considerable scientific interest. The chemist to the Survey, Mr. J. C. Newberry, B.Sc., has discovered phosphates widely distributed in Gippsland, not apparently so concentrated as to furnish an article of commerce, but sufficiently plentiful to impart great fertility to the soil. Some yellowish grey phosphatic soils, hitherto neglected, have been cultivated on Mr. Newberry's recommendation, and have given excellent results.

THE MONTHLY

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APRIL, 1879.

I. IS NATURE PERFECT?

We have often thought it might be useful to exhibit, in all their well-marked contrast with the results of modern Science, those views of Nature which still prevail even among the cultivated classes, and which are very slowly retiring from the fields of literature. For this purpose let us take a man,—such as may be found in abundance in the middle and upper ranks of society,—well-educated, heedful, thoughtful, and refined, but not trained as an observer, and having no special acquaintance with recent discoveries in natural history. Let us give him a holiday, and send him for a summer ramble in the New Forest, in "merrie Sherwoode," or among the ferny coombs of Devon, and let us try the while to read his musings. Even his very first expression of feeling, the sigh of relief on finding himself rid of city-bustle, worry, and noise, is mainly the outcome of an illusion. He fancies himself in a sphere where boundless resources are dispensed with a liberality equally boundless. The heavens are full of light and warmth, and the earth is clad in rich and varied hues. Perfume breathes from every spray. On all sides is life, animal and vegetable, unworn by toil and unshadowed by care and anxiety. To the butterfly hovering over the blossoms, to the blackbird warbling on the spray, the world seems not as to man, the task-yard of a workhouse, but the banqueting-hall of a palace. The observer, even whilst he envies the insects and the flowers, who "toil not, neither do they spin," feels soothed and refreshed by the mere reflection of their supposed felicity.

Plenty requires peace as its natural complement, and our wanderer believes that he finds this boon also in the wood-lands and the heaths. Indeed, how should it be otherwise? Where there is superabundant plenty, where every demand is more than satisfied, how should there be the thrusting, and pushing, and jostling, outward visible signs of that internecine war of each against all, and of all against each, which the "friends of peace" worship under the name of competition? The observer cannot, indeed, forget that in his realms of concord and repose, pain and death are present. He knows that at any given moment hundreds of flies must be struggling in the snares of spiders; that caterpillars innumerable are being gradually devoured by internal parasites; that many a song-bird is falling a prey to the hawk or the weasel. But, as was the case half a century ago even with eminent naturalists, he scarcely apprehends the full meaning of all this suffering and massacre. Such facts as we have just enumerated seem to him mere "rude exceptions to the general joy," departures from the order of Nature, casual, even though numerous, rather than as they really are, part and parcel of its very essence. Our friend in his ramble, and in his necessarily hasty survey, fails to perceive that not only does one-half the animal kingdom live only in virtue of the death of the other half, but that the herbivorous creature is as much a life-destroyer as the beast of prey, extirpating other animals by depriving them of food, and plants by consuming their seed or their seedlings. He overlooks the silent, quiet, but not the less deadly war waged by plants among themselves, each seeking to monopolise to itself soil, air, and light, and to crowd out, starve, or smother its competitors. In short, in his optimist contemplations, he entirely forgets that struggle for existence which—whether or not we regard it as a main factor in the development of animal and vegetable forms—we are bound to accept as perhaps the greatest, and assuredly the saddest, feature of the organic world. Who, after reading the third chapter of the "Origin of Species," can fail to be reminded of those words of St. Paul "For we know that the whole creation groaneth and travaileth in pain together until now;" * or of the sadder exclamation of one who, having no faith in the ultimate solution of this dark riddle, cries out in agony "Creation is murder!" †

But not only is strife rather than peace the order of the organic world,—strife so thoroughgoing and so widespread that it rages even among spermatozooids; not only do the

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* Romans, viii. 22.

† Winwood Reade, Martyrdom of Man.

majority of seeds and ova, from one or other cause, fail to be developed; not only does every species press hard on its means of subsistence, or on the space where alone its being is possible. Even in matters where life is not directly concerned we find Dame Nature not lavish, not liberal, but more penurious than the wife of the thriftiest peasant-proprietor in rural France. Those colours which so fascinate the poet or the artist, and which seem to be spread in such royal lavishment over copse and meadow and heath, have all their purpose to fulfil; they have to serve as an attraction to insects which effect the fertilisation of the flower. The beauty and the odour which we so much admire appear only when this task is necessary, and when it is accomplished they are again withdrawn, just as at a banquet the lights are quenched and the decorations taken down when the guests have departed.

To a sensitive mind it must be saddening to find that the woods, the fields, and the solitudes offer no soothing contrast to the exchange, the workshop, or the battle-field, and that on earth peace, repose, and harmony exist nowhere. But it is the duty of Science to "perceive and declare" whether the facts and the laws recognised be joyous or grievous.

In one sense, indeed, Nature may be called lavish. But it is an unkindly prodigality. She is reckless of life; reckless and wasteful, too, of heat, the prime condition of organic existence. Passing over the fact that the bulk of the solar radiations travel out into the desert depths of space, while an infinitesimal portion alone falls upon any of the planets, very much of the heat which reaches our earth, at least, is radiated off again during the night. Carbonic acid gas, indeed, possesses the precious attribute of admitting the sun's rays freely, and of being at the same time almost impervious to heat-rays of low tension, such as those given off by the earth. But this gas forms but a very small proportion of our atmosphere, and could not be sensibly increased on account of its injurious action upon higher animal life. But if the non-poisonous gases oxygen and nitrogen had the same power as regards the radiation of heat, the climate of the world would be much improved, and spring frosts—the bane of the farmer and the gardener—would be rendered impossible. It is of course conceivable that some cause may exist which renders it impossible for oxygen and nitrogen to possess this attribute without forfeiting their characteristics in other respects.

There is another feature which, outside of scientific circles, we hear commonly ascribed both to the individual animal or plant, to the fauna and flora of any given country, and to the animal and vegetable kingdoms in their entirety. We refer to the attribute of "perfection." We must confess ourselves utterly at a loss to know how this notion has been reached. We have asked believers in this doctrine to tell us by what marks this perfection is to be recognised. We have invited them to take up a plant or an animal, and to demonstrate that any departure from or addition to its present standard, whether structurally or functionally, would be injurious. But the only answer we have received has been a cloud of generalities. The very idea of perfection seems to us unthinkable in reference to a crowd of species engaged, as all Nature is, in mutual conflict. It is by dint of the imperfections of the Carnivora in speed, strength, or cunning that their prey—say the deer or antelope—escapes. It is in virtue of the imperfections of the latter animals that they are captured by the wolf or the leopard. Again, were all animals and vegetables perfect in themselves and in reference to their surroundings, we may ask how it comes that so many species have been exterminated, and that others are even now in course of extermination? It may be contended that the surroundings have altered. This, then, is an admission that the adaptation to circumstances is not always perfect. But further, some other species, or at least groups, coeval with such as have disappeared, are still found surviving. Here we have consequently the following riddle to solve:—Two groups of "perfect" animals, each in "perfect" harmony with its surroundings, are given. These circumstances being altered, the one group is no longer in harmony, and consequently perishes, while the other remains equally well adapted to a different set of conditions, and survives! Yet more; admitting the perfection dogma, we must suppose the fauna and flora of any region—say New Zealand—better adapted to its soil and climate, and to all other local conditions, than any strange animal or plant can be. On introducing such strange species we should therefore see them placed at a disadvantage, and without constant human aid and supervision prove unable to exist at all. But in reality the very reverse is the case; the new comers are not only found able to exist independently of man's assistance, but to spread in opposition to his most strenuous efforts, and even to crowd out the natives. The notion, therefore, that every local fauna and flora forms a perfect whole, perfectly adapted to the circumstances in which it is placed, must be given up as a most glaring error.

Again, we often see large groups of closely-allied species, differing but little from each other, inhabiting the same country, dependent upon the same kind of food, and exposed to the same enemies. Thus there are in Britain alone fifty-seven species of the small dung-feeding beetles included under the genus *Aphodius*. Some of these are exceedingly abundant, others comparatively rare. If all these species are perfect, and perfectly adapted to their environments, why should some be so much more plentiful than others? But, descending more closely to particulars, we may show that in animals, as in man himself, there are certain *desiderata*—wants which Nature has left unsupplied. How exceedingly uncomfortable should we, for instance, feel if we were suddenly deprived of the power which we now enjoy of excluding the light from our eyes when we think proper! Yet as regards the sense of hearing we labour under a similar deficiency; it might rather be said under a greater, since to all persons who have occasion to concentrate their thoughts upon some given subject noise is a far greater nuisance than light can ever be to a healthy man. Surely, then, our inability to render ourselves temporarily and voluntarily deaf is a proof that we, in one respect at least, fall short of perfection.

We may take another instance: what a great addition would it be to man's comfort if he were personally offensive to all insects of the Dipterous order, so that they would keep aloof from him in disgust! When we consider that the mosquito, in addition to the positive irritation, annoyance, and want of sleep which its attacks occasion, is now proved to be an agent in the spread of leprosy,—when we remember that the common house-fly is a propagator at least of ophthalmia, and probably of all zymotic disease,—we surely cannot dispute that such a change, either in the nature of our cutaneous emanations or in the tastes of these pests, would be an incalculable boon, lacking which we cannot proclaim ourselves physically perfect.

Did we know the necessities of other animals as well as we know our own, we might doubtless find in like manner defects on all hands. But we have surely said enough to lead the advocates of organic perfection—who often find in this dogma an *à priori* objection against Evolution—to pause and reconsider the evidence upon which it is based.

The last of the popular assumptions concerning Organic Nature which we can here notice is the old, but still rampant notion that every plant, every animal, exists with reference to man, and for his convenience. "What use is such or such a creature ?" is a question too commonly asked —and not by children only—of the zoologist or the botanist, When we reply that to the best of our belief and knowledge the animal or plant in question is of no use, but simply an unmitigated nuisance, the most charitable construction put upon our answer is that we are too proud to confess our ignorance. Like the doctrine of organic perfection, this dogma is not easily traceable to any substantial basis, but, having once become a current article of popular faith, its extirpation is proving a difficult task. To us it appears, in its very essence, irreverent. If the Creator had, *e.g.*, made the mosquito, or the guinea-worm, or the *Lucilia hominovora* to be of service to man, we may depend upon it that they would not have been sources of annoyance. Even a human invention, the produce of exceedingly finite reason, is condemned if, along with certain and even great good, it effects abundant mischief. How much more if the good be problematic, and the evil open and palpable ? Shall we, then, adduce what is notoriously defective as an instance of Divine "contrivance"? One thing we may certainly conclude, viz., that if a maximum of earthly enjoyment and the minimisation of earthly suffering had been the objects of the Creator the world would have assuredly been constituted very differently from what it is. When we know what His objects really were it will be quite time enough to indulge in teleology and to indite "Bridgewater Treatises."

II. THOUGHTS ON OUR CONCEPTIONS OF PHYSICAL LAW.*

By Prof. Francis E. Nipher, St. Louis, Mo.

IN the short time at my disposal I wish to point out some reasons for the more general cultivation of a certain cardinal virtue which is so rare that I fear it has no name. Perhaps the words *Intellectual Modesty* would come as near as any others in expressing what I mean. The world is very full of people who are ready to make assertions upon subjects which are evidently too difficult for them—in many cases too difficult for any one—to handle with any degree of certainty; and it doubtless often happens that some who have meditated studiously for years upon some such subject, arriving at no satisfactory conclusion, are regarded as objects of profound commiseration by others, who rush upon conclusions like the unthinking horse into the battle. It is as natural that people should thus differ as that some should have darker skins, taller frames, or more irritable tempers than others. To what extent these, and other differences which we shall point out, are blameworthy, we cannot attempt to discuss, but shall study the mental habits of men in precisely the same spirit in which we would study the habits of other animals. But I wish to show some tangible reasons for thinking that there are very few subjects upon which we can dogmatise, and that in any case it is unnecessary. I wish to advocate the cultivation of intellectual modesty, not merely because it seems to me to be one of the brightest ornaments of the human mind, but because of its vital connection with another cardinal virtue—intellectual honesty.

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* Abstract of an Address before the Alumni of the State University of Iowa, June 19, 1878; delivered at Kansas City, December 23, 1878, and which appears in the "Kansas City Review."

Perhaps this end will be best attained by considering the difficulties which are met in the investigation of any subject, when the sole aim is to find out the truth of the matter, and I have thought it proper to point out some of the difficulties to which we are subject in arriving at our conceptions of physical law.

The study of physical science has endowed the human mind with an attribute which is usually ascribed to and is thought to be characteristic of the Divine Mind. I refer to the power of prophecy. The astronomer can predict the position of the planets for generations to come, basing his predictions on the assumption—an unproved assumption—that only those causes which he has considered will act in the future, or, in other words, that the present order of things will continue. His power of prediction does not, however, extend indefinitely into the future, for there are doubtless many minor disturbances or perturbations, too small to be detected by the instruments which he can command, without many centuries of observation, the effects of which will become plainly apparent after the lapse of ages, —that is to say, his power of prophecy is limited by his ignorance of certain facts, and possibly by his inability to solve the equations involved in a complete discussion of the subject.

It is for precisely the same reason that we cannot foretell the future destiny of every person in the world. But to a mind possessing all knowledge, and of infinite power, the one problem would evidently be as simple as the other.

Not only are we unable to predict for an infinite future, on account of the summing up of disturbances which cannot be detected in a short time, with our means of investigation, but, as before suggested, events wholly unexpected to our partially instructed minds—apparent breaches of continuity—are liable to happen at any time.

"A great explosion on the sun may scorch us into cinders in a second. The earth may be dashed to pieces and dissipated into gas, by collision with some immense meteorite. We may become involved in a nebulous atmosphere of combustible gas, which would ignite a moment later; in fact, as was so eloquently pointed out by Mr. Babbage, there is no catastrophe too great or too sudden to be consistent with the reign of law and the continuity of action."

In the discussion of physical phenomena we always ignore the greater part of the discussion, by neglecting those elements which are, or are supposed to be, unimportant. In so simple an operation as the weighing of a quantity of matter on a steelyard, we can discuss only the merest elements of the case. The student of physics would tell you that the weights are inversely as the lever arms, but this is far from being the whole story. During the weighing, certain parts of the steel bar are heated; other parts are cooled; still other parts retain their temperature unchanged; electrical currents are set up within its mass; its magnetism is changed; its torsion and elasticity become different: in fact, to discuss all the changes occurring within the bar during so simple an operation would infinitely transcend the power of the most gifted men.

If we could discuss completely the laws which govern phenomena we should find them represented, in many cases, not by the comparatively simple formulæ which have been found sufficient for practical purposes, but by infinite series, the first terms only of which our mathematicians have been able to deduce, and our physicists to experimentally detect.

In the study of Physics our most certain experimental results force us to ideas equally beyond our power of realisation. It is shown, beyond question, that light moves over a distance of about seven times the circumference of our earth in a single second. We must look for something marvellous in any theory which can account for so marvellous a fact. According to Newton's theory, we should have particles of light shooting off from a distant luminous body with this immense velocity, and, falling upon a mirror, their motion would not merely be checked, but the elasticity of these light particles must be assumed to be so perfect that they rebound with an equal velocity.

According to the undulatory theory, the light consists of vibrations of a medium which fills all space. Since the velocity of transmission of these vibrations is so great it follows that the elasticity of this medium must be 10,000,000,000 times as great as that of the hardest steel. Space is not now regarded as a void, but is filled with a medium which, as Thomas Young remarked, "is not only highly elastic, but absolutely solid." And yet, as we walk through space, the solid atoms which compose our bodies experience not the slightest resistance. Such ideas, although they can be conceived, cannot be realised. We have had no previous experience with materials possessing such properties, and such ideas must necessarily appear strange to us; but they are no more strange than the phenomena of light which we directly observe, and which force us to this or to some other theory equally marvellous. Only those who have carefully examined the subject can realise how weighty is the evidence in favour of the undulatory theory of light; but where such stupendous conceptions are involved, a slavish acceptance of any theory, even by them, would be in the highest degree objectionable. We are not the friends of theories, but of truth.

It is not surprising, then, that in the progress of our sciences many errors of reasoning and in the interpretation of facts have been committed. You are all familiar with the ideas of Newton in regard to the nature of light, ideas which were not in themselves absurd, which were firmly believed in by this man of such transcendent power, but which were clearly negatived by results of subsequent experiment.

Lavoisier's idea that all acids were compounds of oxygen received a complete refutation when the constitution of prussic and muriatic acids became known. In fact the errors of scientific men are well-nigh innumerable, not because they are men of science, but because they are *men*, and we are probably justified in saying, quite in general, that if the man who never committed a mental blunder be found, we shall also find a man who never conceived a vigorous thought. The fact that the results of scientific men can usually be checked by observation and experiment perhaps diminishes their liability to err, and enables them to discover multitudes of errors that would otherwise escape their attention. This does not tend to make the results of their investigations less weighty than results which have been reached by other processes more purely mental. If men of science, with their severe methods of research, their habits of testing their conclusions by observation and experiment, are nevertheless led into wrong conclusions, what does it prove? Simply that the human mind, even under the most favourable circumstances, is fallible! Is there a class of men less liable to make mistakes? It is precisely this experience which causes many to place a small value upon the unsupported assertions and speculations of any man, however honest, earnest, or able he may be.

On this point one of the most admirable of experimenters, Faraday, has beautifully said—"The world little knows how many of the thoughts and theories which have passed through the mind of the scientific investigator have been crushed in silence and secrecy by his own severe criticism and adverse examination; that in the most successful instances not a tenth of the suggestions, the hopes, the wishes, the preliminary conclusions, have been realised."

In the 24th series of his "Experimental Researches" Faraday describes many tedious and intricate experiments, in which he tried to connect gravitation and electricity. "He laboured with characteristic energy for days, on the clock-tower of the Houses of Parliament and in the shot-tower of Southwark, raising and lowering heavy weights connected with wire coils. Many times his great skill as an experimenter prevented him from being deceived by results which others would have regarded as conclusive proofs of his idea, and when the whole was done there remained absolutely no result." For although the results were wholly negative, Faraday could never accept them as conclusive against his idea, to which he had been led by his experiments on the relations between electricity and magnetism. His mental condition after this work was done is best described in his own words:—"Occasionally, and frequently, the exercise of the judgment ought to end in *absolute reservation*. It may be very distasteful, and great fatigue to suspend a conclusion; but as we are not infallible, so we ought to be cautious."

It is a matter of common observation that men who, like Faraday, have done much to widen the boundaries of our knowledge, are precisely the ones who are most frequently in a state of doubt, while those who have received all their knowledge at second hand are generally more ready with a positive decision and a reason for it, not necessarily because their intellectual integrity is less, but because they *cannot* realise how vain a thing the human reason is. To imagination and reason, controlled and checked by experiment and observation, are we to look as the source of the greatest advancement in science; but we are not to look for infallibility, and in cases where the reason alone is allowed to decide, where observation and experiment are impossible, the uncertainty must necessarily be greater. In many cases the fact that the subject is so intrinsically difficult that no experimental check is possible, appears to inspire the investigator with a confidence in his conclusions that could hardly be reinforced by absolute certainty.

I have confined myself to the errors which scientific men have committed, and to which they are liable in their search for truth, not because they alone are liable to err, but because a discussion of the multitude of errors into which intellectual men of other professions have fallen would be sure to give offence. But it is not the scientific mind which stands impeached—it is the *human* mind!

To what end have we then come? It appears that all scientific results are attended with some uncertainty. Sometimes the uncertainty is very small, and we are able to obtain a numerical estimate of it. In other cases it may be possible that a fundamental misconception of the truth may have been formed. As an instance illustrating what I mean we may cite the case of the measurement of the Atlanta base-line by the engineers of the United States Coast Survey. The whole length of the base-line was nearly 6 miles, and three determinations of its length showed differences of about 3-10ths of an inch—about a millionth of the entire length. It is safe to say that if these re-measurements had shown differences of 10 feet there would have been no quarrelling in regard to which measurement was right, but all would have been rejected; and if the engineers were not dismissed as incompetent, they would, with feelings of mortification, have begun their work over again. It is curious to observe that in many cases where less skilful men attack problems infinitely more complex, reaching conclusions differing as widely as the poles, we have, instead of conscientious re-investigation or a modest reservation of judgment, dogmatic discussions, empty words. In the other case, where the error is likely to be a fundamental one, the probability of the truth or error of a conclusion cannot always be determined numerically, and will vary greatly in different minds.

In conclusion, allow me to say that it seems quite probable that human welfare does not require us to sit in judgment on the ideas of others. Thoughtful men are becoming more and more impressed with the vastness of the unknown and the comparative insignificance of human achievement, while the demonstrated fallibility of human reason leads them to temperance and modesty of thought and expression: to *appreciation*, as well as toleration, of opposition and doubt. Certain it is, that if we preserve our intellectual integrity we shall be unable to settle many of the problems that interest us most. If we decide upon some of them, and other persons still reserve their judgment or decide differently, we need not lose our tempers; they have not only decided differently from us, but we have also decided differently from them. It is important to notice that neither of these decisions has affected the *truth* in the least. If we feel called upon to defend the truth, we are, after all, only defending what we *believe* to be truth, and possibly against men as honest and as able as ourselves. But why should we defend the truth? So long as the heart of humanity shall pulsate, will not truth be cherished there? Why would it not be far better for each one to put himself in the attitude of a reverent *searcher* for truth? remembering always that the little decisions that we may reach are possibly wrong, that all of the honesty and ability in the world is not concentrated within ourselves and the comparatively few who think as we do, and that one can do nothing nobler than to make himself as intelligent and humane as possible, resolutely following out his highest convictions, and living at peace with himself and with all men.

III. THE OLD STANNARIES OF THE WEST OF ENGLAND.

By James Quick.

IT is a remarkable fact, though unnoticed by most writers of English history, that there existed in England for many centuries an assembly which held the regulation or supervision of all matters connected with what was for a long period the most important branch of English commercial enterprise. Yet such was the case. The privilege of making laws for the direction of tin mining in the West of England was enjoyed by two convocations designated the Parliaments of the Stannaries, and which were composed of representatives elected by the miners or "tinners," as the workers in the Cornish and Devonshire mines were until within about the last fifty years commonly called. A Stannary Parliament used to meet at a place called Hengiston Down,* subsequently at Lostwithiel, and later on at Truro, to legislate for the Cornish tin-mines; and another at Crockern Tor,† and in the middle and later ages at Tavistock, for those of Devonshire; the Members of the Parliament for the former county being styled "Stannators," and those for the latter "Jurats." These Parliaments, which in some form appear to have existed from time immemorial, and probably originated in gatherings of the miners to discuss the prospects of their trade, and later on perhaps used to meet to demur at the enormous and oppressive dues levied from them by the Crown, acquired in the Middle Ages a very large amount of power over the mining interest. To become law a Bill for regulating mining affairs required to be signed by each Member of the Stannary Parliament before which it was brought, and then by the Lord Warden of the Stannaries, and by the Sovereign or Prince of Wales in his capacity of Duke of Cornwall, and as such the head of Stannary affairs.‡ During the last three centuries, too, the King or Duke could not make any law for the Stannaries without the consent of the twenty-four Members of either Parliament,‖ and in 1752 (see note p. 284) it was enacted that a Bill brought into the Cornish Stannary Parliament required, to become law, to receive the assent of sixteen members of the Parliament.

For the administration of the laws made by the Stannary Parliaments there were at different places Stannary Courts,§ and "in all matters of complaint the tinners were judged by their own laws before the Judges of the Stannaries," and they were "exempt from all other jurisdictions of the kingdom in all causes within the Stannaries amongst them, except in such as affected land, life, or limb." They were also exempted from serving on juries other than those connected with the Stannaries, from paying tithes to the clergy out of their wages, and could sell their their own goods at fairs or markets without paying any fees. The Stannary Courts were held every three weeks, and a Court was held once every month by a Vice- or Sub-Warden to hear cases brought from the other Courts; and from the decision of this Court there was no appeal, save to the Lord Warden, the Duke in Council, or the King in person.

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* Near Callington. † On Dartmoor.

In the eighteenth century Mining Committees were occasionally opened at these places and adjourned to a Stannary Court town. (See note § below.)

‡ This was according to an old Charter of Edward III., which was the most important and implicit of any in its grants to tinners.

‖ This was according to a Charter granted by Henry VII., and called the Charter of Pardon. Much of the information above given about the Stannaries has been taken from "A State of the Proceedings of the Convocation or Parliament for the Stannaries of the County of Cornwall, held at Lestwithiel, Jan. 29, 1750, &c." by a Cornishman; London, 1751 (see pp. 2 and 3). No Parliament appears to have previously met since 1710. See also Sir Jno. Dodridge's "History of the Ancient and Moderne Estate of the Principality of Wales, Duchy of Cornewall, &c." London, 1630.

§ The old Stannary Court and Coinage towns in Cornwall were Launceston, Lestwithiel, Truro, and Helston, and, after 1811, Penzance. In Devonshire they were Plympton, Ashburton, Tavistock, and Chagford. There were Stannary prisons at Lidford and Lestwithiel. The Stannary districts in Cornwall were Foweymore, Blackmore, Tywarnhaile, Penwith, and Kerrier, each of which sent six members to the Stannary Convocation.

The history of Stannary matters prior to the seventeenth century, owing chiefly to many valuable records having been destroyed during the Civil War, is very obscure, but during that and the following century the Stannary Parliaments were extremely active. The memberships appear to have been regarded as posts of considerable importance, and to have been eagerly sought after, possibly with some sinister motive of ulterior gain, for during those centuries a large amount of underhanded dealing in Stannary affairs seems to have been practised. The writer of an account of a Cornish Parliament which met in 1750 (see note ‖ page 283) remarks that "during the time of an election gentlemen think it worth while to come down into the county who were never seen in it at any other time, and to neglect their business and the pursuit of their affairs for three months together, without having or pretending to have the least knowledge of tin or Stannary matters." The same writer also remarks "that whoever will take the pains to search into it will find the constitution of the Stannaries to be as well established, both by Charter and Act of Parliament, as any particular constitution can be, or even as the constitution of the kingdom itself." Little could he have thought that, within another century from the time he wrote, the Stannary Parliaments, together with most of the ancient customs and privileges of the tinners, would have fallen into desuetude, and that the Stannary Courts would have merged into a single restricted tribunal, with every prospect of that being soon combined with the County Court. Probably the last of the Stannary Parliaments was the one that assembled at Truro on 13th September, 1752.* In

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* The laws made by this Parliament are given in "Laws of the Stannaries of Cornwall, &c.," published by order of the said Parliament, 1752. In 1752, also, the laws made 22 Jac. I., 12 Car. I., and 4 Jac. II. are confirmed.

1837 the different Courts were consolidated into one Court, which now periodically meets at Truro, and exercises in many respects similar jurisdiction over Stannary matters to that of the Courts of Chancery, Common Law, and Equity over others. The right possessed by the monarch of pre-emption, or of buying any tin raised in the two counties at his own price and in preference to anybody else, and often exercised in the Middle Ages to a very oppressive extent, was, together with many other Stannary prerogatives, abolished by two Acts passed in the reign of William and Mary,* whilst the dues on tin payable to the Duchy of Cornwall were only cancelled so late as the year 1838.†

One of the oldest and most extraordinary privileges of the West of England tinners was the very widely observed custom of "bounding," which may perhaps remind our readers of the practice of "beating the bounds" still observed by many municipal authorities in England. In Cornwall and Devonshire all land was formerly considered either as being in "severall" or in "wastrell," the former term being applied to lands enclosed or built upon, and the latter to those lying waste or open. By the "bounding" custom any person might enter upon the "wastrell" of another, and mark out by four "boundaries" any certain area. A written description of the land bounded, and the name of the "bounder, had then to be recorded in the nearest Stannary Court, and a notice given to the owner of the soil. The "bounding" had then to be proclaimed in three successive open Courts, and if no objection were raised against it a writ‡ was given to the bounder, investing him with the exclusive right to search for and appropriate to his own use all tin or tin-ore found within the described limits, upon the condition that he pay to the landowner (in Cornwall) a fifteenth part in kind of the quantity of tin raised. The four corners of the land bounded were generally marked by a pole being erected at each, with a furze-bush at the top, and it is told amongst the miners that on May-day mornings the poles used to be decorated with flowers,[*] and that youths and maidens used to dance around them.

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* 1 Wm. and Mary, c. 30, and 5 Wm. and Mary, c. 36.

† By Act 1 and 2 Vic., c. 102, which substituted for the dues an equivalent sum payable from the U. K. Consolidated Fund. For further particulars on this point see Appendix to the Stannary case of Vice *v.* Thomas (London: Saunders and Benning, 1843). The Stannary duty in Cornwall from the time of Richard Earl of Cornwall (granted to him by Charter in 11th of Ed. III.) had been 40s. for every 1000 wt. of tin raised, and in Devonshire from time immemorial 15s. 6d. for every 1000 wt. of tin raised. There was also a custom of stamping or "coining" each block of tin raised and smelted in the district with the Duchy arms. This was also abolished in 1838.

‡ Several of these are still in existence. One is preserved in the Museum of Practical Geology, and Royal School of Mines, Jermyn Street.

The right of bounding, which could be disposed of by the possessor in the same manner as other property, might be preserved to an indefinite period, either by actually working the land and paying toll, or by annually "renewing" the four boundary marks on a certain day, by performing the operation of cutting a turf a square foot in size, and piling thereon some loose earth in the form of a mole-hill,[†] at each corner of the land bounded. Neglect to renew the bounds in any one year forfeited the right to them, and evidence before the Equity Law Courts of recent years[‡] has confined the area boundable to narrow limits, although such limits have never been accurately defined. As to the origin of the bounding custom we incline to the opinion of Dr. Borlase,[||] that the privilege was granted for the encouragement of the tinners. Possibly it may have been at first confined to the "wastrell" land of the King, who would, of course, derive benefit from such land being worked for tin, and afterwards became applied to all wastrell. It soon, however, grew to be a very oppressive custom for the ordinary landowner. The injustice of one man being able to prevent another from raising the tin that might be in the latter's ground was certainly very great, and it is a matter for surprise that anyone upon coming into possession of land in Cornwall did not at once take up his own bounds, for we cannot find any Cornish Stannary Law which confined the holding of bounds to the tinners. The bounding custom in Devonshire differed from the custom of Cornwall in that the tinner could search in "severall" as well as in "wastrell," and was not compelled to pay any toll or compensation to the lord of the soil. Tin-bounding in the two counties has now, however, been in disuse for many years, and no mine at present worked is held under its tenure.

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[*] It is stated in a footnote at p. 318, vol. iii., of BRAND's Antiquities, that the poles were decorated with flowers on St. John's Day. An old miner told the above-given particulars to the writer of this paper.

[†] See PRYCE, Mineralogia Cornubiensis.

[‡] Further particulars of the bounding custom in the West of England will be found in a Treatise on the Laws relating to Mines, by R. P. COLLIER, Barrister-at-Law, &c. (London, 1849), which work is a valuable digest of Stannary Law. See also Britain's Metal Mines, by J. R. PIKE (London, 8vo., 1860); also PRYCE's Mineralogia, pp. 137—139, and BORLASE's Natural History, p. 167. The Charter which principally confirmed the privilege was one of 33 Edward I. (A. D. 1305), and others referring to the custom are still extant.

[||] See his Natural History of Cornwall, Oxford: 1758.

IV. A NEW THEORY OF TERRESTRIAL MAGNETISM.

By Profs. PERRY and AYRTON.

AT the meeting of the Physical Society on the 7th inst. Mr. Ayrton explained, on behalf of Prof. Perry and himself, a theory of terrestrial magnetism which has the great novelty that it makes the existence of the earth's magnetism depend solely on the earth's daily rotation, and does not require, as do all other theories based on electro-magnetic phenomena, the existence of other bodies in the universe. In fact, they have arrived at the result that if any body of any material has a static electric charge, and if it rotates about an axis, then *per se* there will be a magnetic field in the interior of this body as well as in the neighbourhood outside.

In 1876 Mr. Rowland, working in Professor Helmholtz's laboratory, proved experimentally that a quantity of electricity in mechanical motion acted like an electric current in deflecting a magnet, and it is on this result that Profs. Perry and Ayrton have based their whole theory. For they point out that since the points near the surface of the earth have different linear velocities from those in the interior (although all the points have the same angular velocity of rotation round the earth's axis), it follows that if the earth had an initial electrical charge, residing of course in accordance with the well-known electrical law, on its surface, the electrified particles would have velocities relative to the remainder; hence, as a direct consequence of the results of the experiments published by Professor Helmholtz, the interior of the earth would be a magnetic field, quite independently of its interior constitution, and precisely similar reasoning, of course, proves that outside the earth's surface there would also be a magnetic field.

In order to solve the difficult problem of determining the electro-magnetic potential at any place, they have calculated the magnetic forces produced at any point, first by the rotation of a small electrified area on the surface of the earth, and then, by summation, the force produced by the rotation of the whole electrified surface; and they have shown that these forces are the same as would be produced by certain definite distributions of attracting matter over the earth's surface, from which, by the use of spherical harmonics, they have arrived at the conclusion that if the earth had an angular velocity of rotation around its axis w, and a uniform electric density σ, then there must be an electro-magnetic potential at any point inside the earth equal to—

$$\frac{4\pi}{3} 2\sigma w r \cos\theta,$$

and at any point outside the earth—

$$\frac{4\pi}{3} 2\sigma w \frac{1}{r^2} \cos\theta,$$

where the unit of length is the earth's radius, r the distance from the earth's centre of the place in question, and θ its co-latitude.

Now this electro-magnetic potential will be accompanied with certain magnetic forces varying from point to point, and the magnitude of these forces will depend on the interior constitution of the earth. As an example of their conclusion they examine what would be the distribution of magnetic intensity if the earth consisted of a hollow iron shell, and they have, using Poisson's formula, arrived at the following result:—If such an iron shell has an initial uniform charge of static electricity, and if it has an angular velocity of rotation round a diameter, then, independently of all other bodies in the universe, and independently of the coefficient of magnetisation of the iron, there will be at any point on the surface of the sphere, having a latitude λ, a magnetic force proportional to—

$$\sqrt{1+3\sin^2\lambda}.$$

But this result is the same as that given by Biot's well-known law for the distribution of magnetic intensity on the earth's surface, hence giving considerable probability to their theory.

It is important to notice that in previous examinations of the earth's magnetism it has been usual to start with the known law of distribution of magnetic intensity, and then deduce what arrangement of magnets, &c., inside the earth would lead to this distribution; but in this new theory of Profs. Perry and Ayrton they start merely with an experiment described by Prof. Helmholtz, of the effects of a rotating electrified body, and show from this that the earth by its rotation alone *must* be magnetic, and next prove that if there be an iron shell, thick or thin, in the earth then the distribution of magnetic intensity on the earth's surface will be the same as is known to exist from observation.

Next as regards the sign of the electric charge on the earth's surface required to produce the earth's magnetic polarity—Is it in accordance with the known phenomena of atmospheric electricity? To produce the earth's magnetism we must have, in accordance with the known laws of electro-magnetism, a *negative* current flowing from west to east or in the direction of rotation of the earth. In the language of the new theory, therefore, the surface of the earth must be *negatively* charged, but Sir William Thomson has proved, by observations with his electrometer, that all the phenomena brought to light by atmospheric electricity, on a fine day, would be observed just as they are if the earth had a *negative* charge.

Lastly, in order to get a rough approximation of what sum be the difference of electric potential between inter-planetary space and the earth so that its own electric charge alone combined with its known rate of rotation round its axis shall produce the earth's magnetic moment, as determined by Gauss, Profs. Perry and Ayrton take as an example a solid sphere of iron of the size of the earth, and rotating with the earth's known rate of rotation, and they prove mathematically that something like a difference of potential a hundred million times the electromotive force of a single Daniell's cell would be sufficient for this purpose. They notice that there is no difficulty in imagining such a difference of potential to exist between the earth and interplanetary space, seeing that there exists between the earth and the planets an enormous region of space having an insulation far higher than that of such a vacuum as experiment has shown will not allow the passage, from one point to another very near it, of a spark produced by a large induction coil.

Lastly, they draw attention to the fact that they have assumed the distribution of the electric charge on the earth's surface to be uniform, and so have arrived at a distribution of magnetic intensity dependent only on the latitude of the place, but that since it is possible that the sun and planets may have potentials differing immensely from that of the earth, it might be expected that the distribution of electric charge on the earth's surface, and consequently the magnetic intensity, would have variations like those of the tides, in fact such variations as are known to exist; also, that it would be anticipated that any sudden formation of vapour on the earth's surface, or alteration of the sun's atmosphere, or anything causing change in the lines of *static electric induction* from the sun to the earth ought to cause disturbances such as we know as magnetic storms. The alignments of planets with the sun and earth, again, or the proximity of planets to the sun having less than the average difference of electric potential from it, ought to diminish the disturbances in the solar envelope and produce the alteration of sun-spots and terrestrial magnetism which is known to accompany such planetary motions. And, lastly, the authors point out that the known lagging of changes in terrestrial magnetism behind variations in the sun-spots would be explained if the great pressure to which the iron in the earth's interior is subjected produces, as is very probable, considerable coercitive force, for such coercitive force would necessarily cause changes in the magnetic intensity to lag behind the disturbances in the earth's electric charge produced by alterations in the static induction of the sun and planets.

V. THE JABLOCHKOFF CANDLE; ITS PRACTICAL RESULTS IN LONDON.

By CHARLES W. QUIN.

IT is now nearly three-quarters of a century since Davy first exhibited to an astonished and delighted audience, at the Royal Institution, a voltaic arc, of no less than 4 inches in length, playing between the terminals of the great 2000-plate battery with which he obtained illuminating and heating effects exceeding anything of the kind which had been heard of up to that time. For many years the most brilliant of terrestrial lights received no practical application, owing to its defects being numerous and apparently insurmountable. As to its adaptation to the wants of everyday life no really serious move was made in the right direction until within the last few years, up to which period it had only been used for lighthouses, building works carried on at night, a few factories, and some startling theatrical effects at the Paris and Vienna Operas, the efforts to apply it to street-lighting and other general purposes being sporadic and generally unsuccessful. Many of its original defects have long since disappeared, but much still remains to be done. The rapid waste of the charcoal terminals was obviated by Foucault in 1842 by the use of hard gas-retort carbon. The gradual weakening and ultimate cessation of the current, and the proportional diminution and final extinction of the light, were got rid of by the inventors of the different forms of constant batteries. The unequal wearing away of the two poles, and the consequent flickering and frequent extinction of the light, were almost entirely triumphed over in 1846 by Staite, who invented the first regulator or electric lamp. The enormous expense caused by the rapid consumption of zinc in the battery was reduced to a minimum by the application of Faraday's great discoveries in magnetic electricity by Holmes, who made the first offer of his machine to the Brethren of the Trinity House in 1857, and their ultimate acceptance of it for lighthouse purposes on the recommendation of Faraday formed a new era in the history of the light.

The next important step in the development of the subject was the great increase of magneto-electric force obtained by means of the Siemens armature, in which the wire was wound longitudinally instead of transversely. Perhaps the most important step of all was the discovery—made apparently independently by Siemens, Wheatstone, and Varley—that the small amount of magnetic force contained in every bar of wrought-iron was capable, by action and reaction, of producing heating and lighting effects far superior to those obtained with artificial magnets. Another difficulty, the expense involved in having to provide a certain number of artificial magnets which were constantly losing strength, was now done away with, the cost of the current being thereby still further reduced.

In 1876 M. Denayrouze startled his colleagues of the French Academy by laying before them an invention of a young Russian officer which he designated an electric candle. Until then no one had thought of keeping the tips of the carbons always at the proper distance by placing them side by side. This was the most simple form of lamp yet invented; there was no clockwork to get out of order; there were no trains of wheels to stick when they ought to move; no delicate but capricious magnets; but simply a couple of brass sockets, with a double-stemmed candle, consisting of two rods of carbon separated by a seam of kaolin, thrust into them.

This is neither the place nor the time to inquire whether the Jablochkoff candle is better or worse than any other form of electric lamp. Be this as it may—and the truth can only be arrived at after long experience—the Jablochkoff candle was the means of bringing the question of electrical illumination before the world in a very prominent manner. It was first applied in Paris to the illumination of the showroom of the Magasins du Louvre and the adjoining courtyard, the Place and Avenue de l'Opéra, and the Hippodrome, thereby becoming the most prominent system of electric lighting known to the general public. From Paris its fame spread to London, and within the last three or four months its capabilities for public use have been put to the test in the metropolis in four different places—the Thames Embankment, the Holborn Viaduct, the British Museum Reading-Room, and the fish-market at Billingsgate, under the able direction of M. Berly, the agent of the Paris Société Générale d'Electricité.

The experiment on the Thames Embankment is the most extensive and important that has yet been tried in England, both with respect to the number of lamps used and to the elaborate data which are being collected with regard to its cost, its virtues, and its defects. The portion of the Embankment which is lighted is that lying between Westminster and Waterloo Bridges, a distance of about three-quarters of a mile. The experiments have been carried out by the Metropolitan Board of Works under the direction of their engineer, Sir Joseph Bazalgette, and their consulting chemist, Mr. T. W. Keates, who have both determined that as far as in them lies the experiments shall be thoroughly exhaustive and their results conclusive. The engine and machinery for generating the currents are placed in a wooden shed on the west side of the Charing Cross railway-bridge, the conducting-wires being led under the roadway to the subway which runs under the footway next the parapet-wall of the Embankment, and so up to the lamps, which are carried by every alternate gas standard on the wall. The engine, which is by Ransome, Sims, and Head, is one of 20-horse power nominal, working at about 45 lbs. pressure. It is provided with a newly-invented governor, acting on the expansive principle, and has worked uninterruptedly from the 16th of December until the present time without a single hitch. It is to the extreme regularity of this engine and to the solid foundation which has been laid for it that we must attribute the great superiority in the steadiness of the Jablochkoff lights on the Embankment as compared with those shown elsewhere. The dynamo-electric apparatus consists of a Gramme continuous current exciting machine and an alternate-current distributing machine of 20-light power. The number of lamps is twenty, divided into four circuits of five. They are, with one exception, supported by the gas standards on the river parapet, and, roughly speaking, are from 40 to 45 yards apart. From the engine-house to the farthest lamp on the Westminster side the distance is about 700 yards, and to that on the Waterloo side about 450 yards. Each lamp contains four candles, which are switched into their places as they are required by an automatic commutator, the holders being surrounded, with one exception, by opal glass globes 20 inches in diameter. Two lamps near the railway bridge are provided with flat reflectors, 4 feet in diameter, placed on the top of the globe so as to throw the light downwards. One of these reflectors is turned inwards at an angle of 30 degrees for the space of 6 inches all round. The lamp under the bridge is provided with a very coarsely frosted globe, which shuts off much less light than the opal globes. Each candle lasts one hour and a half; the lamps therefore burn for six hours every evening.

The three months' trial ended on the 16th of March, but the Board of Works, determined that the experiments should be as complete and conclusive as possible, have renewed their contract with the Société Générale d'Electricité for an additional period of three months. It is rumoured that before long the whole of the Embankment from Westminster to Blackfriars will be illuminated by Jablochkoff candles. This could easily be done by providing an extra pair of dynamo-electric machines to work in the shed, the engine being at present worked at less than half its strength.

About the beauty and efficiency of the electric light, as applied to the illumination of the Embankment, there can be only one opinion, and that a most favourable one. The sternest opponent of the electric light, on account of its "weirdness," its "ghastliness," and so on, need only stand in the middle, and glance alternately at the Victoria and Albert Embankments, to be thoroughly converted, unless he be a gas engineer in what theologians call "a state of invincible ignorance." With men like Sir James Bazalgette and his colleagues controlling the results we may be sure of obtaining data that will be absolutely unprejudiced and indisputable, and their report will be awaited with impatience by all who take an interest in the great battle of the lights.

The City Commissioners of Sewers have acted very differently to their brethren of Spring Gardens, and have refused to carry on any more experiments in lighting the Holborn Viaduct, their engineer and surveyor, Colonel Haywood, having apparently frightened them by reporting that although the electric light was seven times as bright as gas, it was seven and a half times as dear. These city Solons would evidently like to buy diamonds at the price of paste, or shall we say real turtle at the price of mock? According to Colonel Haywood's Report, made on the 28th of February, to the Streets Committee of the City Commissioners of Sewers, arrangements were entered into in November last with the Société Générale d'Electricité for lighting the Viaduct from the western end of Newgate Street to Holborn Circus, with sixteen Jablochkoff candles, for a period of three months, beginning on the 14th of December.

Fifteen of the lamps were placed at alternate intervals on each side of the road, and one in the centre of the carriage-way at the junction of Newgate Street with the Viaduct. As in the case of the Embankment the existing lamp-posts were utilised, the gas lanterns being removed, and replaced by the Jablochkoff candles and their accompanying opal glass globes. Measured crosswise the distance between the electric lamps was about 110 feet, or about one-tenth farther apart than the electric lamps on the Embankment. The light from each lamp, according to Colonel Haywood, covered an area of 888 square yards of public way. The lamps were lighted at sunset and continued burning until midnight, and replaced eighty-six gas-lamps, sixty-one of which were lighted at midnight. The electric lamps were fed by two Gramme machines of a similar pattern to those used on the Embankment, driven by a Robey engine of 20-horse power nominal, the whole of the machinery being contained in a temporary shed erected on a piece of vacant land on the western side of Farringdon Street, close by the Viaduct. The lamps were arranged in four circuits of four each, and the conducting-wires were laid in tubes beneath the road and carried up inside the lamp-posts to the candles. The arrangement of the commutators was the same as on the Embankment.

With respect to the cost of the experiment, from the 14th of December to the 18th of February, the figures given by Colonel Haywood are as follows:—The Company agreed to provide, fix, and fit up the engine, machinery, conductors, and lamps for £236 8*s*. 4*d*., and the Sewers Commission agreed to provide the shed and its enclosures and to do all the work underneath the roadway for about £267. From this must be deducted £50 10*s*. 10*d*. allowed by the Gas Company for gas not burnt. In case of sudden extinction—which happened on four different occasions—a lamplighter of the Gas Company was in constant attendance from sunset to midnight (an average of seven hours per night), at the comfortable wages of a few pence over £2 per week, which can hardly be considered exorbitant considering the onerous nature of his duties. The sixteen lamps were lighted from sunset to midnight at a charge of £5 per night, the average time which they have been alight during the experiment being (according to Colonel Haywood) about seven hours. The total cost of the experiment therefore, after deducting the saving in gas, cleaning the lamps used, &c., was £785 6*s*. 5*d*. The rest of Colonel Haywood's calculations we need not enter into minutely. He tells us that, at

£5 per light for the sixteen lamps, the cost per lamp is 10s. 7*d*. per hour, or about £3072 per annum for 4300 hours; the eighty-six gas-lights originally used costing during the same period £419, which is about two-fifteenths of the major sum. Colonel Haywood seems to have made no photometric experiments on the Viaduct light; his estimate that the sixteen electric lights give out about seven times more light than the eighty-six gas-burners is based on the observations of others. It will be seen from this that Colonel Haywood's data of the relative value of the two kinds of light are somewhat hazy; it would be interesting to compare his figures with those contained in the report of his *confrère*, M. Cernesson, Surveyor to the Paris Municipality, but the exigencies of space prevent it.

The lighting of the City thoroughfare was as successful in its way as that of the Embankment; the passage of the wayfarer from the yellow gloom of Newgate Street into the "sweetness and light" of the Viaduct was hardly more striking than his emergence out of the gentle moonlight of the Jablochkoff candles into the darkness visible of Holborn.

The third experiment was a highly successful one, and proved most satisfactorily that by employing a sufficient number of lights the British Museum Reading-Room might be made available to readers by night as well as by day. Fifteen out of the nineteen reading-desks were lighted by eleven candles, enclosed in an opal globe, and placed on a pedestal 15 feet high, fixed in the exact centre of each desk, a twelfth lamp being placed in the centre of the room: four of the desks were consecutively lighted, the remainder alternately. The reading-room was kept open until 7 o'clock on the evenings of the 3rd, 4th, and 5th of March, in order that the new light might be thoroughly tested in a practical manner by those for whose benefit the experiment had been made. The readers showed their hearty approval of the innovation by remaining behind in considerable numbers. On the first night a strange incident occurred. At a few minutes before 6 o'clock, when the twelve Jablochkoff candles suddenly flashed forth, the readers, led by a well-known poetess, and forgetting they were in the reading-room, evinced their admiration of the beauty of the light by a burst of applause—a sound hitherto unknown under that dome of silence.

Mr. E. A. Bond, the Principal Librarian of the British Museum, to whose initiative these experiments are due, has been at some trouble to collect information as to the opinion of the readers, and there appears to be a general agreement that at the four desks each lighted with a candle it was perfectly possible to read, write, trace, draw, and even colour, with the greatest ease and comfort. The source of electricity on this occasion was a 20-light Robey portable engine of 16-horse power nominal. Four circuits of four lamps each were used,—that is to say, twelve in the reading-room, one in the entrance-hall, one in the portico, and two in the machine and engine shed. This experiment was remarkable for being the first occasion of the introduction of an improved Jablochkoff candle, in which the kaolin seam is replaced by a composition which is a feeble conductor. One of the great defects of the electric light, re-lighting by hand after sudden extinction from accidental causes, is thus entirely done away with. The use of carbon priming for lighting or re-lighting is no longer necessary, and one, two, three, or the whole of the four candles may be lighted or extinguished by turning the handle of a suitable commutator, or if one goes out it lights itself automatically without extinguishing its neighbours.

The Société Générale d'Electricité deserves great credit for the public spirit they have shown in gratuitously supplying everything necessary for making this interesting experiment, which will be carried into practice as soon as the consent of Parliament has been obtained.

The experiments at Billingsgate were a complete failure, from a somewhat ludicrous cause—the Jablochkoff candle was literally too good for the work it had to do. Business at Billingsgate, as most people know, begins between 3 and 4 in the morning, most of the bargains between the salesmen and their customers being struck by gaslight. If the beautiful goddesses and fairies of the stage that used to enthral our hearts in the days of our youth had been suddenly transferred from the yellow glow of the footlights into the open daylight the disenchantment would have been painful—Titania would have become converted into a Hecate, and Venus into a Megæra. The Billingsgate fish suffered a similar transformation under the searching rays of the electric light: soles that would have fetched a shilling a pair by gaslight looked dear at sixpence, while turbot fresh from the sea looked a week old. The result was a general outcry. The copious and ornate dialect of the locality was enriched by a number of notable additions during the few days that the electric light had sway; so, for fear of a revolt among the "bummarees," as the fish salesmen are called, the Corporation was obliged to restore to them their beloved yellow gaslights, and, through no fault of its own, the Jablochkoff candle know the sons and daughters of King Belinus no more.

VI. THE TORNADO AT WISCONSIN IN 1878.

NEARLY five years ago* we gave as full a description as could be obtained of a tornado which occurred in Iowa and Illinois on May 22nd, 1873. Singularly enough a storm, accompanied by a tornado extending nearly across Wisconsin, which for extent and violence surpassed any storm previously recorded in the history of the State, swept over Iowa, Northern Illinois, and Southern Wisconsin only one day later of the same month in 1878. We are indebted to Prof. W. W. Daniells, of the University of Wisconsin, for a complete account of the tornado of May 23rd, 1878, and we regret that the space allotted to a single article in the "Monthly Journal of Science" will only permit of a very brief abstract of his description of its character and extent.

With regard to the origin of the storm it has been impossible to obtain data leading to any definite conclusion. At about six o'clock of the same day a tornado from a south-westerly direction struck the earth near Barrington, in the north line of Cook county, Ill., passing over Highland Park half an hour later. The same evening at half-past six a tornado occurred north-east of Quincy, Ill., in Adams and Brown counties. The occurrence of these several tornadoes, together with the fact that heavy rains fell throughout eastern Iowa, northern Illinois, and southern Wisconsin on that afternoon, shows that the Wisconsin tornado was not entirely the result of local conditions, but that some cause producing unusual atmospheric disturbance was widespread. The weather chart accompanying the Monthly Weather U.S. Signal Service indicates an area of low barometer for May 23 extending over the entire Lake region.

The evidence obtained by Prof. Daniells seems to prove conclusively that the tornado was a whirlwind of unusual proportions, having its motion of revolution in a direction opposite to that of the sun, or from the east to the north, west south, to the east again.

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*See Quarterly Journal of Science, vol. v. (N.S.) p. 339.

According to the most reliable information as to the rate of the forward movement of the storm, it struck the earth at about 3 p.m., between which time and 5.30 p.m. it passed over a distance of sixty-four miles, which would be at the rate of twenty-six miles per hour. Any conclusions upon the velocity of the wind within the tornado can, as Prof. Daniells points out, only be reached by indirect methods. The wind's rotatory velocity south of the axis would be increased, while north of the axis it would be diminished by the progressive movement of the storm. The amount of increase or diminution would, however, differ greatly in different parts of the whirl. That the velocity of the motion of revolution increased from the outer edge toward the centre is shown by the fact that buildings near the centre were reduced to the dimensions of kindling wood in a way which never occurred near the borders of the track. Hence the horizontal velocity of each particular volume of air was continually changing as it occupied different positions relative to the axis and to the centre of the whirl. Again the wind had an upward motion, the perpendicular component of which acted upon bodies to hold them in suspension. The velocity of this upward motion also increased toward the centre of the whirls, and was comparatively slight near its limits. The actual velocity of the wind at any point would be the resultant of the horizontal and perpendicular velocities at that point.

The perpendicular velocity can be approximated by the lifting force exerted upon bodies while holding them in suspension. For instance, a horse weighing about 1100 lbs. was carried over twenty rods. Another of about the same weight was carried about eighty rods. A horse of this size would not expose a lifting surface to the wind of over 14 square feet. To lift such an animal would require then an upward pressure of the air of $\frac{1100}{14} = 78{\cdot}5$ lbs. per square foot. This pressure is produced by wind moving with a velocity of 124·6 miles per hour. In the township of Jefferson a granite boulder 15 in. square and more than 6 in. in thickness, and weighing more than 130 lbs., was carried 15 rods. To hold this rock in suspension would require a lifting force of 852 lbs. per square foot of surface. This force would be exerted by wind moving with an upward velocity of 129 miles per hour. These examples show the great velocity with which the wind moves in such a storm.

There were three distinct storm paths west of Rome, Jefferson county, which is as far as Prof. Daniells's personal observations extend. It was, he remarks, impossible to trace any connection upon the surface of the earth between the different storms, although the ground between them was all passed over and most diligent enquiries made. The evidence gathered in regard to the time of occurrence of these different tornadoes shows conclusively not only that they were distinct but also that the second and third were contemporaneous with the eastern portion of the first one, and also that the three were dissipated very nearly at the same time.

The effects of these tornadoes as pourtrayed by Prof. Daniells was most disastrous. In the City of Mineral Point alone the amount of damage is estimated at 3,904,500 dols. The largest individual losses in this city were those of Mr. Gillman and Mr. John Spensley, respectively 20,000 dols. and 11,000 dols. The house of Mr. John Spensley, a large new frame house, with all its contents, was torn to fragments and swept away, some portions having been found a mile distant. Twelve persons were in the house, all but one of whom were in the cellar. Mrs. Waller, who remained to find a child she supposed had not gone to the basement, with the family, was instantly killed. No one in the cellar was injured. Mr. Spensley's barn and carriage house were completely demolished, and his carriages and sleighs broken past repairing, yet a horse standing in the farm escaped without injury. Another house belonging to Mr. Spensley, occupied by George Leonard, was destroyed, and Mrs. Leonard instantly killed. Seven other houses and five barns were utterly destroyed, and four houses and a large brewery unroofed and otherwise injured. Mrs. Myers and Mrs. Bohan were killed, as their respective residences were destroyed, and several others received injuries. Three horses and some other stock were killed within the city limits. Had the storm continued the course it was on, half a mile west of the city, it would have passed for three quarters of a mile over a part of the town very largely populated, in which case the loss of life would have been very great, and the suffering and destruction of property many times greater than it now is.

As the storm passed on its way a school house containing fifteen children and the teacher was destroyed, everything above the floor being carried away. The teacher was carried about 15 rods, but not materially injured. Two children were killed, one injured, the remaining escaping without injury. The school record was found next day over sixty miles distant.

At about three quarters of a mile from the township of Waldwick the barn and blacksmith shop of Stephen Terrell were razed to the foundation. A waggon and threshing machine and many other farm implements were broken, a corn cultivator was carried twenty-five rods and completely demolished. Mr. Terrell's house, near the centre of the storm's path and in a very exposed position, was scarcely injured. Seeing the storm coming some distance across the prairie, Mr. Terrell hurried his family into an embankment cellar, remaining outside himself until it was but a short distance away. He described the cloud as reaching to the ground, the lower part so black and opaque that nothing could be seen within it, while at a height of 200 or 300 feet the air appeared to be filled with trees, rails, boards, hay, leaves, and other débris, all rapidly whirling, and shooting upward and downward in terrible commotion.

The storm passed into the township of Primrose. The house and outbuildings of M. Obermbt were swept away. The house was torn to pieces and scattered to the south and south-east. Mr. Obermbt and seven children were in the house at the time, and were thrown into the yard with the flying fragments of the house. One boy, fifteen years of age, was carried about fifteen rods nearly south into a ravine. Although the ground was so thickly strewn with the ruins as to be literally covered for 100 yards to the south and south-east, no one of these eight persons was seriously injured. The farm waggon before the storm stood six rods east of the house. After the storm it was in ruins twelve rods west of the house. Fifty rods south of Obermbt's, where a granary was being built, a waggon loaded with lumber was broken to pieces. One wheel was carried a quarter of a mile directly east, and another a mile and a quarter in the same direction.

Nearly half a mile east of Obermbt's the house and outbuildings of J. Osmonson were destroyed. Mr. Osmonson seeing that a severe storm was approaching, left the field where he was at work, that he might not get wet. Becoming somewhat alarmed at the roaring, the continuous lightning and thunder, and the very threatening aspect of the sky, he waited at the stable only long enough to unharness one horse, hurried into the house and told his wife they must hasten to the cellar. A boy of fourteen years and a girl of eight got into the cellar, and Mrs. O., with an infant three months old, were partly down when the house was taken bodily. At this time Mr. Osmonson, with a child in each hand, aged respectively four and six, stood at the cellar door waiting for the mother and her babe to get fully down. Besides these, there was in the house a girl twelve years old. This girl was found thirty yards distant north of east, senseless, nearly buried in mud, with two severe scalp wounds, and her right arm broken, three times between the shoulder and elbow. About four rods north of the house was the border of a large field of second growth oak and poplar timber, from 20 to 40 feet in height. The house was carried over the timber, with Mr. Osmonson and the two children, whom he still held firmly in his grasp. While in the air over this timber the house "went to pieces," the larger portion of it falling sixteen rods directly north of its starting point. One portion of the roof was twenty-five rods distant in a direction north 30° west, and another portion sixty rods distant north 25° east. The stove was mainly found seven rods directly north of the principal ruins of the house; some parts, however, were carried several rods farther in the same direction.

Mr. Osmonson and the two children fell about 20 ft. north of the main ruins of the house. Mr. Osmonson had his face scratched and one rib broken in falling through the top of a tree. The children were entirely unhurt, the youngest one did not even cry. Large hail was falling at the time, and the children were laid under the ruins of the house while the father hastened to find the other members of the family. The children in the cellar were not hurt. Mrs. Osmanson was injured in the back, probably by something striking her as the house moved off. The stable in which the horses had been put was eight rods south-west of the house. One of the horses was blown into the cellar, and lay there upon his back when found, while the other was in the standing timber, twenty-two rods distant, north 38° east from the stable, with his hind feet resting upon the ground, while his fore feet were hanging upon a bent over sapling. The position of the horse, and the thick growth of timber rendered it impossible for him to get there only by being carried above the tops of the trees and dropped down. He was uninjured. An iron pump with 46 feet of zinc pipe was taken from a well, and carried north 15° west, a distance of fifteen rods. A lumber waggon was broken entirely to pieces. One wheel and an axle was carried north 65° east, seventy-five rods, while the larger proportion of the remainder went north-east sixteen rods. One wheel was entirely broken to pieces and the tire left hanging on a tree 10 feet from the ground. This tire, ½ in. thick, 1 and 1½ ins. wide, and very slightly worn, was broken twice in two and bent in such a manner as to show that it had been acted upon by a force of great power.

Eighty rods north-east of Osmonson's stood a house belonging to Mrs. Ketchum. This house was on the south side of a hill. It was taken bodily from the foundation, up the hill north, and left in a little niche in the woods north-west from its starting point fifteen rods. The family escaped by going to the cellar.

The storm bent to the north at this point, destroying everything in its course, and scattering the débris east, west, north, and south. The deed of a farm, the house and two barns of which were destroyed, was found ten miles distant. A portion of an organ in the house was found 4½ miles directly north, while the boiler and some cooking utensils were carried east one mile. On the bank of Sugar river a granary and log house were destroyed. The stove, a part of the furniture, and some of the logs of the house were blown into the river. A lady school teacher boarding at the house was saved from the same fate by a log falling upon her and holding her down. And this work of destruction proceeded until in Jefferson county the storm entered an open marsh and was dissipated.

It has often been noticed that the severity and destructive violence of tornadoes is much greater in some portions of their path than in others. In the present case there are instances of buildings remaining uninjured while strong ones near by were destroyed. In many places where there was continuous timber there would be strips from ten to thirty rods long in a direction parallel to the axis of the storm, where nearly every tree was prostrated, then an interval where little damage was done, and again another piece where all were down. Tracts of interrupted violence frequently reached entirely across the track of the tornado, but they usually extended only partly across. In Prof. Daniells's opinion they were more frequent north of the axis than south.

VII. THE ELECTRIC LIGHT FOR INDIA.

IN the report on the results obtained by the electric light experiments, instituted on behalf of the Board of Directors of the East Indian Railway Company, Mr. Louis Schwendler states that in endeavouring to determine the quantity of light per unit of power, unit of speed, and unit of money, he tried four different dynamo-electric machines producing the electric current in *one* direction, viz., a medium and a small-sized Siemens (Hefner von Alteneck) dynamo-electric machines; a dynamo-electric machine, workshop pattern, as supplied by Messrs. Soutter and Lemonnier, of Paris; and a dynamo-electric machine with two sets of brushes, as supplied by the British Telegraph Manufactory. The trials have established that these four machines are all sufficiently practical for the production of the electric light, and that the unit of light as produced in the electric arc by any of the four dynamo-electric machines is at least fifty times cheaper than the unit of light as produced by combustion, considering the expenditure of power only. This represents an enormous engineering margin in favour of the electric light.

The Siemens medium dynamo-electric machine, however, gives about double the quantity of light of any of the other three machines, and only about half as much power is expended to produce the unit of light. This favourable result is principally due to the comparatively small internal resistance of the machine, and to its low speed.

For use in India Mr. Schwendler proposes several alterations in the machine.

With regard to lamps, Mr. Schwendler for practical use prefers the Serrin lamp, with the following alterations, which his own experiments have suggested :—

1. All parts of the lamp, including clockwork, &c., to be made of gun-metal. No steel or iron is to be used except in the electro-magnet and its armature.

2. The carbons may be of any shape—round, triangular, or square. The carbon-holders, constructed as in the Siemens lamp, should be sufficiently large to hold a round carbon of 18 m.m. diameter. With 18 m.m. carbons the lamp should burn eight hours.

3. The screw for regulating the tension of the spring or springs which act in opposition to the magnetism of the

electro-magnet should move 1 m.m. by one whole turn. In order to know the adjustment at any one time, and to be able to make the *same* adjustment again, a millimetre scale is to be attached, by which the movements of the screw can be read. The circumference of the head of the screw is to be divided into ten equal parts. Hence by this arrangement the comparative tensions of the spring or springs can be read up to 0·1 m.m. This micrometer screw, after the lamp has been regulated for any given current varying within two known limits, is to be fixed by a clamp and screw to keep that adjustment *constant*.

4. The distance between the electro-magnet and its armature is also to be made adjustable by a micrometer screw, provided again with millimetre scale like the above, and with a clamp and screw, for making any best adjustment *constant*.

5. The clockwork and all regulating parts of the lamp to be entirely covered by a strong metal case, which is to be constructed in such a manner that it can be taken off or put on without interfering in any way with the adjustment of the lamp. The case may be cylindrical, opening like a door in two halves on hinges, and with a key for closing it. None of the adjustable parts of the lamp are to protrude, as it is intended not to touch the lamp after its proper adjustment, which is done in the laboratory only. Each lamp has only *one* best adjustment for any given current varying between two known limits, and the best adjustment is made constant by fixing everything. The case or cover is then closed, and the lamp put up for use.

6. The electro-magnet which pulls the arc should offer no more resistance than 0·02 S.U. It is to be *shunted* by another electro-magnet which offers exactly the same resistance. Hence one-half of the current passes through the electro-magnet of the lamp, and is made use of *for pulling the arc*. The amount of iron used in the electro-magnet, and the number of convolutions, should be such that at the mean distance of the electro-magnet from its armature the magnetic force is strong enough to produce an arc of 2·5 m.m. against the mean tension of the spring or springs when employing a current of about 25 Webers. The magnetic action of the shunt for the same current should be about double that of the electro-magnet of the lamp, in order to leave a margin for a finer adjustment, *i.e.*, equalisation of the magnetic action of the two. The two electro-magnets, each forming a shunt to the other, are adjusted in such a manner that the extra currents they produce, when the

primary current varies, are equal, and therefore, as they are invariably opposite to each other, they neutralise one another entirely, which will have the desired effect of a quicker regulation of the lamp for any variation of current. The iron used in the shunt should have double the weight of the iron in the electro-magnet. The section of the wire for filling the shunt should be double the section of the wire filling the electro-magnet. Coil on so many convolutions on to the shunt until its resistance becomes equal to the resistance of the electro-magnet.

For adjusting the quality of the extra currents the following method should be adopted :—

Form a Wheatstone bridge two sides of which are formed by a mercury rheostat, each side offering about 0·02 S.U. resistance. The third side of the bridge is formed by the electro-magnet of the lamp, the fourth side by the shunt. In one diagonal place a dynamo-electric machine and about *one unit* resistance, together with a convenient make-and-break contact, best done by a mercury cup. In the other diagonal place a Bell telephone of lowest possible resistance. One end of this diagonal can be moved along the mercury rheostat. Start the dynamo-electric machine, listen to the telephone, and alter the ratio of the mercury branches of the bridge, by shifting along the contact until the telephone is perfectly silent. Then if, at commencing and stopping the current, a strong click is heard, we know it is due to the two extra currents not being equal, and as we further know that the shunt produces the greatest extra current we make this extra current smaller, by shifting along the two poles of the shunt an iron wedge, until the telephone is quiet when starting and stopping the current. The iron wedge is then fixed in its position. This shunt is also to be *inside* the metal cover of the lamp.

7. The two terminals of the lamp are to be of exactly the same pattern and size as those used in the dynamo-electric machine. They must *not* be terminals with hand-screws, but strong hexagonal-headed screws with lock-nuts.

The *division of the electric light*—*i.e.*, the production by the *same electro-motor* of a number of lights at different points of a given space—Mr. Schwendler considers to be impracticable from an engineering point of view. Such divisions of the electric light can, he argues, only be effected by a large sacrifice of total and external light, and moreover this loss increases rapidly with the number of lights burned in the same circuit. He succeeded in working three Serrin lamps connected up successively in the circuit of a dynamo-electric machine, but found the loss of light very great. It appears that the electric light can alone compete with light produced by combustion when produced of great intensity in *one* point by *one* dynamo-electric machine.

Having satisfied himself of the difficulty and impracticability of the division of the electric light, he tried *diffusion*—*i.e.*, a few large lights (each light produced by one machine) are placed at different points of the space, and by optical means the light is diffused over a large area. This method is found to be perfectly practicable. There is naturally also a large amount of light lost (by absorption), but this loss will bear a constant ratio to the total light produced, and probably may decrease with the intensity. The light is diffused by means of a *silvered glass* reflector in which a powerful electric light burns, throwing *direct* and *reflected* rays up to a white ceiling, or any other convenient white surface. The form and size of each reflector must of course depend on the locality where it is to be used.

Mr. Schwendler recommends the employment of the electric light at railway-stations in India. The station is to be lighted up with four powerful electric lights, placed judiciously, so as to have the greatest and most uniform effect, each light being produced by *one* lamp and *one* dynamo-electric machine. Each light, by an opaque silvered glass reflector, is thrown against a white ceiling or any other white surface. No *direct* rays are allowed to enter the retina. This, Mr. Schwendler says, produces a light effect, resembling daylight, with no shade whatsoever. The intensity of the light will be such that the smallest print can be read by a normal eye at any place on the platform.

VIII. PAINLESS DEATH.

IN one of his lectures Prof Tyndall spoke of the probabilities in favour of the entire absence of pain accompanying death by lightning. It is popularly supposed that an impression made upon the nerves—a blow or puncture—is felt at the precise instant it is inflicted, but such is not the fact. The seat of sensation is the brain, and intelligence of the injury must be transmitted to this organ through a certain set of nerves, acting as telegraph wires, before we become conscious of pain. This transmission or telegraphing from the seat of injury to the brain requires *time*, longer or shorter, according to the distance of the injured part from the brain, and according to the susceptibility of the particular nervous system operated upon.

Helmholtz, by experiments, determined the velocity of this nervous transmission in the frog to be a little over 85 feet per second; in the whale, about 100 feet per second; and in man, at an average of 200 feet per second. If, for instance, a whale 50 feet long were wounded in the tail, it would not be conscious of the injury till half a second after the wound had been inflicted. But this is not the only ingredient in the delay. It is believed that to every act of consciousness belongs a determinate molecular arrangement of the brain, so that, besides the interval of transmission, a still further time is necessary for the brain to put itself in order for its molecules to take up the motions or positions necessary to the completion of consciousness. Helmholtz considers that one-tenth of a second is required for this purpose. Thus, in the case of the whale, there is, first, half a second consumed in the transmission of the intelligence through the sensor nerves to the brain, about one-tenth of a second consumed by the brain in completing the arrangement necessary to consciousness, and, if the velocity of transmission from the brain to the motor nerves be the same as that through the sensor, about half a second more is consumed in sending the message to the tail to defend itself. Therefore one second and one-tenth would elapse before an impression made upon its caudal nerves could be responded to by a whale 50 feet long.

If we regard as correct the calculations representing the average velocity of transmission in the human nerves, and if we estimate the distance from the origin of the filaments in the brain to their termination in the foot as 5 feet, the time required, in case some one steps on your favourite corn, for the news to be telegraphed to the brain, for the brain to prepare a message, and to telegraph the same to the muscles of the leg to draw the foot away, would be about one-twentieth of a second. Now, it is quite conceivable that an injury might be inflicted which would render the nerves unfit to be conductors of sensation, and if this occurred, no matter how severe the injury might be, there would be no consciousness of it. Or it might happen that the power of the brain to complete the molecular arrangement necessary to consciousness would be wholly suspended before there would be time for the transmission of the intelligence of the injury. In such a case, also, although the injury might be of a nature to cause death, this would occur without feeling of any kind. Death in this case would be simply the sudden negation of life, without any intervention of consciousness whatever.

Doubtless there are many kinds of death of this character: the passage of a rifle-bullet through the brain is a case in point. The time required for the bullet in full velocity to pass clean through a man's head may be roughly estimated at a thousandth part of a second. Here, therefore, would be no room for sensation, and death would be painless. But there is another action which far transcends in rapidity that of the rifle-ball. A flash of lightning cleaves a cloud, appearing and disappearing in less than a hundred-thousandth part of a second, and the velocity of electricity is such as would carry it in a single second of time over a distance almost equal to that which separates the earth and moon.

A luminous impression once made upon the retina endures for about one-sixth of a second, and this is why we see a ribbon of light when a glowing coal is caused to pass rapidly through the air. A body illuminated by an instantaneous flash continues to be seen for the sixth of a second after the flash has become extinct; and if the body thus illuminated be in motion, it appears at rest at the place where the flash falls upon it.

The colour-top is familiar to most of us. By this instrument a disk with differently coloured sectors is caused to rotate rapidly; the colours blend together, and, if they are chosen in the proper proportions, the disk will appear white when the motion is sufficiently rapid. Such a top rotating in a dark room and illuminated by an electric spark appears motionless, each distinct colour being clearly seen. Prof. Dove has found that an illumination by a flash of lightning produces the same effect. During a thunderstorm he put a colour-top in exceedingly rapid motion, and found that every flash revealed the top as a motionless object with its colours distinct. If illuminated solely by a flash of lightning, the motion of all bodies on the earth's surface would, according to Prof. Dove, appear suspended. A cannon-ball, for example, would appear to have its flight arrested, and would seem to hang motionless in space as long as the luminous impression which revealed the ball remained upon the eye. If, then, a rifle-bullet passing through the brain move with sufficient rapidity to destroy life without the interposition of sensation, much more is a flash of lightning competent to produce this effect. We have well-authenticated cases of people being struck by lightning who, on recovery, had no recollection of pain.

The Rev. Dr. Bartol, who was lately nearly killed by lightning, expressed the belief that if the stroke proved fatal it must produce the most agreeable mode of death; but to be stunned, as he was, is very unpleasant. As soon as consciousness returned he experienced a terrible sense of oppression, and an irresistible weight seemed passing through him, while his mind was dazed so that for a while it seemed he had suddenly been precipitated into wonderland. His recovery was attended by headache, continued for a week.

The following case is described by Hemmer;—On June 30, 1788, a soldier in the neighbourhood of Mannheim, being overtaken by rain, stationed himself under a tree, beneath which a woman had previously taken shelter. He looked upward to see whether the branches were thick enough to shed the rain, and in doing so was struck by lightning, and fell senseless to the earth. The woman at his side experienced the shock in her foot, but was not struck down. Some hours afterward the man recovered, but remembered nothing about what had occurred, save the fact of his looking up at the branches. This was his last act of consciousness, and he passed into the unconscious condition without pain. The visible marks of a lightning stroke are usually insignificant, the hair being sometimes burnt, slight wounds occasioned, or a red streak marking the track of the electric discharge over the skin.

Prof. Tyndall relates—standing in the presence of an audience, about to lecture—that he accidentally touched a wire leading from a charged battery of fifteen large Leyden jars, and the current passed through his body. He says life was absolutely blotted out for a very sensible interval, without a trace of pain. In another second or so consciousness returned. He saw himself in the presence of the audience and in contact with the apparatus, and immediately realised that he had received the battery discharge. The *intellectual* consciousness of his position was restored with exceeding rapidity, but not so the *optical* consciousness. To prevent the audience being alarmed he stated that it had often been his desire to receive, accidentally, such a shock, and that his wish had at length been gratified. But while making this explanation the appearance which his body presented to himself was that of being in separate pieces. His arms, for example, seemed to be detached from his body and suspended in the air. Memory and the power of reasoning and speech were complete long before the optic nerve recovered from the electric shock. The Professor dwelt upon the absolute painlessness of the shock, and believes there cannot be a doubt that to a person struck dead by lightning the passage from life to death occurs without consciousness. It is an abrupt stoppage of sensation, unaccompanied by a pang.—*National Medical Review.*

NOTICES OF BOOKS.

Notes by a Naturalist on the Challenger, *being an account of various Observations made during the Voyage of H.M.S.* Challenger *round the World, in the Years* 1872-76. By H. N. MOSELEY, F.R.S. London: Macmillan and Co.

WE have here a general summary of the zoological and botanical results of the *Challenger's* vogage. It is remarked in the Preface that "very much of it was intended for family reading," and in fact no small portion of the book might—as far as internal evidence goes—have been written by any observant man of culture having no special acquaintance with any department of Natural History. We find even the music of the Tahitian National Air, a modern production which can scarcely be regarded as an ethnological document. It must further be remembered that, except as far as strictly Oceanic life is concerned, the circumstances of the Expedition were not too favourable to extended biological observation. The proportion of time spent on shore was not large, and, as the author declares, the zoological results of the deep-sea dredgings were disappointing. Hopes had been entertained, by the late Prof. Agassiz and others, that at great depths many important forms of old geological epochs would be found still existing. Such expectations have not been fulfilled; and though many new species, and even new genera, were obtained by the dredge, they belonged as a rule to well-known families, and very few displayed any important structural difference from forms already recognised. "We picked up no missing links to fill up the gaps in the great zoological family tree."

In another respect the results may be pronounced negative. Upon land wide areas have been found specially favourable to the production of variations and the development of new forms. Hence the faunæ and floræ of continents are relatively richer than those of islands. It might therefore have been expected that the ocean-floor, wider far than any continent and enjoying throughout conditions of temperature essentially alike, would have disclosed a corresponding exuberance of forms. The very contrary is the fact: deep-sea life is monotonous. Nor do the voyagers appear to have been favoured with a sight of any of the still unclassified monsters which the sea, according to some witnesses, may be supposed to contain. In *the* sea-serpent Mr. Moseley appears to be a decided unbeliever, and accepts the usual hypotheses of floating sea-weed, flocks of birds, &c., remarking further that "Sea-serpent stories are often utterly without foundation in fact, and sometimes apparently ships from which they emanate are laden with rum."

On the subject of the visual powers and the colouration of deep-sea species we find some interesting remarks. The author very justly pronounces it quite conceivable that animals might exist to which obscure heat-rays might be visible, and to which men and other mammals would appear constantly luminous. On examining with the spectroscope the phosphorescent light emitted by three species of deep-sea Alcyonarians, he recognised the red, yellow, and green rays only. In connection with this fact it must be noted that "almost all the deep-sea shrimps and Schizopods are of an intense bright scarlet, differing markedly in their intensity of colouring from shallow-water forms, and having, apparently for some purpose, developed an unusually large quality (quantity ?) of the same red pigment which colours small surface Crustacea." Many deep-sea Holothurians are, however, dark purple, and in their case Mr. Moseley considers the colouring useless, and due merely to the persistence of a pigment developed originally in shallow-water ancestors.

Among the more important observations on fishes we may refer to the notice of *Periophthalmus Kolreuteri*, which travels on mud and moist earth, using its pectoral fins as saltatory legs. Concerning flying fish the author agrees with Dr. Möbius, that the species of *Exocœtus* do not flap their fins at all during their flight. As to the *Dactylopteri* (the flying gurnets) he has distinctly seen the fins or wings in rapid movement.

Among terrestrial animals Mr. Moseley appears to have paid the greatest attention to birds. He mentions that the Apteryx of New Zealand "considers it necessary to put as much of its head as it can under its rudiment of a wing, when it goes to sleep"—a very curious instance of the persistency of habits. On the other hand, we find a converse case where part of the penguins on the small island of Inaccessible have learnt to nest in holes under rocks, where the swine cannot get at them. On the north side of the island, where the pigs cannot get, the penguins retain their original mode of nesting on the open ground. The account of the voyagers making their way through a penguin colony is not pleasant reading.

Concerning insects we find fewer observations than might have been anticipated. Referring to a flock of a dozen males of *Ornithoptera poseidon* who were paying their court to a single female, the author proposes some experiments to test hypothesis of sexual selection, both in case of butterflies and birds. "A hen might be kept in a cage between two males, noting to which she gave the preference, and then whether any alteration in the colours of the plumage caused a change in her inclinations."

A starling (*Calornis metallica*) is mentioned as having been found caught in the web of a yellow spider at Little Ke Island, and at St. Thomas a large ground spider (*Lycosa*) is said to habitually prey on lizards. At Juan Fernandez the author was struck with a showy flora, co-existing with a paucity of insects—an observation similar to that said to have been made in the mountains of Guiana by Schomburgk.

Whilst watching the movements of seals in the water, Mr. Moseley was struck by their close connection with the whales, and saw how easily a whale might be developed out of a seal. The fur-seals when on land still bend their hinder limbs forward, as do land mammals. The sea-elephants carry their hind legs always stretched backwards, when little modification is needed to turn these otherwise useless members into the broad tail-fin of the whale.

With reference to the interval between man and the anthropoid apes, we find an utterance with which we heartily agree :—"The wide, but unscientific, distinction commonly drawn between man and the higher monkeys is an error of high civilisation, and comparatively recent. To the Dyack, the great ape of Borneo is simply the Man of the Woods. The author, when at Tonga, was struck with the manner in which the natives, when conversing, incessantly contract and relax the muscles of the forehead and twitch up the eyebrows, just as do the monkeys.

Passing over abundance of other observations well worthy of notice, we come to a passage on the possible faunæ and floræ of other worlds. "On the theory of Evolution it is impossible that plants or animals, of any advanced complexity, at all resembling those existing on the earth, should exist on other planets or other solar systems. It is conceivable that very low forms of vegetable life may exist on other planets, and may have been by some means transported to the earth : the idea is conceivable, though highly improbable. But it is quite impossible that that infinitely complex series of circumstances which on the earth has conspired to produce from the lowest living forms a Crustacean should have occurred elsewhere ; still less is it possible that a bird or a mammal should exist elsewhere ; still more impossible that there should be elsewhere a monkey or a man. It is even probable that protoplasm itself, the basis of all life, is a production entirely confined to our small planet." To all this we must of course assent if—but only if—species are the result of an infinite number of fortuitous variations.

We cannot better convey our opinion of this work than by expressing the hope that the author may soon be engaged on some exploring expedition which may give him greater scope, and that we may have the pleasure of reading the account of his results.

A Monograph of the Silurian Fossils of the Girvan District, in Ayrshire, with Special Reference to those contained in the "Gray Collection." By H. ALLEYNE NICHOLSON, M.D., &c., and R. ETHERIDGE, Jun., F.G.S. (Fasc. I.—Rhizopoda, Actinozoa, Trilobita.) Edinburgh and London: W. Blackwood and Sons.

THE authors have been led to undertake a detailed and systematic description of the fossil remains found in the Silurian area of the Girvan district, which they consider will be an important contribution towards the solution of certain problems in both palæontology and geology. Judging from this first fasciculus they are carrying out their task with scrupulous care, accuracy, and thoroughness.

Studies in Comparative Anatomy. No. II. "Anatomy of the Indian Elephant. By L. C. MIALL, Professor of Biology in the Yorkshire College, and Curator of the Leeds Museum; and F. GREENWOOD, Curator to the Leeds School of Medicine. London: Macmillan and Co.

THE authors have dissected a young female elephant, and have been able to make not a few additions to and rectifications of the descriptions furnished by earlier observers. They have confined themselves almost exclusively to myology. The osteology and dentition they omit altogether, as ordinary text-books already contain descriptions sufficient for the purposes of the naturalist or the palæontologist.

Concerning the alimentary canal and its appendages, and the circulatory, respiratory, and reproductive systems, they give merely a summary of what is already known, supplemented by their own observations.

As regards the nervous system they are unable to give any novel information, the rather as the brain of their subject was not removed till eight months after death, when it was found too much disintegrated for examination.

They recommend those who may have the opportunity to make a microscopical examination of the large intestine of the elephant immediately while fresh, and also to investigate the brain minutely immediately after death. The value of this monograph is increased by the bibliography of the subject.

CORRESPONDENCE.

THE SEA-SERPENT.

To the Editor of the Monthly Journal of Science.

Sir,—With reference to the letter of "Serpent-Hunter," in your February No., I would beg to ask whether anyone has taken the taken the trouble to ascertain whether any such narrative as that quoted in Dr. Wilson's work was really deposed to at Liverpool, or whether the whole is not the invention of some audacious *canard*-monger? A friend tells me that this point has been raised by one of your contemporaries.—I am, &c.,

Sceptic.

SPIDER'S WEB FOR MICROMETERS.

To the Editor of the Monthly Journal of Science.

Sir,—Mr. W. Mattieu Williams, in his article on "Spider's Web for Micrometers," contained in your last issue, speaks of spiders as "his six-legged friends." This is doubtless a clerical error, as spiders have eight legs.—I am, &c.,

An Entomologist.

ADHÉMAR'S THEORY OF EVOLUTION.

To the Editor of the Monthly Journal of Science.

Sir,—Has the possible bearing of Adhémar's theory upon the question of Evolution ever been thoroughly discussed? If every ten thousand years the waters of the globe are translated from the Northern to the Southern Hemisphere, deluging the continents on their way, must not the great majority of animal and vegetable species, terrestrial at least, be extirpated? If prior to the last of these great cataclysms the bulk of the water occupied the Northern Hemisphere, where were the wide continents in which the majority of organic species seem to have taken their rise?

I am well aware that Adhémar's theory is far from being demonstrated ; but is not its doubtful compatibility with the doctrine of Descent at least worthy of consideration ? If the Antarctic ice-cap possesses the dimensions lately ascribed to it in the "Quarterly Journal of Science,"—say a thickness rising from about half a mile at its margin to 24 miles at its centre, and extending over a circle of 3000 miles diameter,—it seems to me that we have here a mass of water which if rapidly set at liberty would go far to render Adhémar's deluges (with the last of which that of Noah is probably identical) at least physically possible.—I am, &c.,

A. P. P.

THE SENSES OF THE LOWER ANIMALS.

To the Editor of the Monthly Journal of Science.

SIR,—In an article on the "Senses of the Lower Animals," inserted in your last year's volume, the alleged fondness of serpents for milk and their power of destroying it by smell are incidentally referred to. In "Science Gossip" a case is mentioned, on the authority of a country clergyman, where a viper followed for some distance a woman who was suckling her infant. Popular tradition in many parts of Europe accuses snakes of fastening on the teats of cows. Now I do not deny that serpents may have an acute scent, as the manner in which the common viper congregates wherever the marsh-rosemary (*Ledum palustre*) flourishes seems to be sufficient affirmative evidence ; but in my own somewhat extensive personal experience I never met with any instance of a snake of any kind being attracted by milk, and I should be happy to hear if any of your naturalist readers have in this respect been more successful.—I am, &c.,

SERPENT-HUNTER.

OPTICAL ILLUSIONS—A CORRECTION.

To the Editor of the Monthly Journal of Science.

SIR,—On page 236 of the March number of your Journal a footnote to my article on "Optical Illusions" states that "An account of a very similar observation was communicated by Mr. J. Aitken to the Royal Society of Edinburgh, in November, 1878, apparently without any knowledge of the observations of Addams, Brewster, or of the author of this article." I have received word from my friend Mr. Aitken that, while the observation was made without any such previous knowledge, references to the older observations were given in the account communicated to the Royal Society of Edinburgh, and Mr. Aitken has courteously permitted me to peruse his paper in confirmation of the fact. I will only add that no mention of previous workers appeared in the abstracts given by the scientific press which came under my notice; and the inference that Mr. Aitken was not aware of the existence of these when his paper was presented to the Society appeared to be confirmed by the title he gave it, describing the phenomenon as "A *New* Variety of Ocular Spectra."

I am happy, however, to make the correction which Mr. Aitken has pointed out to me.—I am, &c.,

S. P. THOMPSON.

University College, Bristol,
March 13, 1879.

PROCEEDINGS OF SOCIETIES.

ROYAL SOCIETY, *February* 13.—A paper by Mr. J. E. H. Gordon, "On an Extension of the Phenomena discovered by Dr. Kerr, and described by him under the title of 'A New Relation between Electricity and Light,'" was read. In November, 1875, Dr. Kerr announced, in the "Philosophical Magazine," that he had discovered a new relation between electricity and light. He showed that when glass is subjected to an intense electrostatic stress that a strain is produced which causes the glass to act like a crystal upon polarised light. While the author was endeavouring, by means of the electric light, to project the effect of Dr. Kerr's experiment on a screen, the electrostatic stress was accidentally allowed to become strong enough to perforate the glass. Immediately before perforation there occurred the following effects:—First appeared a patch of orange-brown light, about 6 or 7 inches diameter. This at once resolved itself into a series of four or five irregular concentric rings, dark and orange-brown, the outer one perhaps being 14 inches diameter. In about two seconds more these vanished, and were succeeded by a huge black cross about 3 feet across, seen on a faintly luminous ground. The arms of the cross were along the planes of polarisation, and therefore were at 45° to the line of stress. The glass then gave way, and all the phenomena disappeared except the extreme ends of the cross, and the discharge through the hole, where the glass had been perforated, was alone seen.

February 27.—"Studies in Acoustics. I. On the Synthetic Examination of Vowel Sounds." By William Henry Preece and Augustus Stroh. In their investigation the authors assumed that vowels are compounded of a prime sound and certain upper partials. Since each partial can be considered as a simple harmonic curve, if we assume the pitch of a prime to be constant, then it would be possible, by means of a machine, to represent and vary each partial in phase and in amplitude. For this purpose an instrument was constructed, which the authors call "the synthetic curve machine," in which a number of toothed wheels are mounted on steel pins or axes rigidly fixed on a board, so that they will revolve together, and the numbers of their teeth are so calculated that during one revolution of the wheel A, B will make two, C three, D four, E five, F six, G seven, H eight revolutions, and so on. Besides assuming the pitch to be constant, the authors assumed that each octave of the partial, to maintain equal loudness of sound, must diminish one-half in amplitude as it rises. This instrument enabled the authors to form synthetically all the curves produced by vowel tones, and to show how these tones are compounded of primes and harmonic upper partials. It shows how *simple tones* can be produced by simple harmonic curves, and *compound tones* by the simultaneous action of several simple tones. The authors then determined to try to reproduce the vowels by sounding a prime and one of its partials alone. This was done by means of an electro-magnet vibrating an armature with a movable spring attached to it in such a way that the vibrations of the armature could produce a given prime, while the vibrations of the spring, by varying its length, could also be adjusted to any particular partial. The effect not being by any means perfect, a machine was made on the principle of the synthetic curve machine, which would, instead of drawing curves on paper, reproduce eight partials by transferring the vibrations of the intermediate wheels to a vibrating diaphragm. Here, again, though the vowels were fairly reproduced, something was wanting in their clearness. Another machine was now made upon which disks were fitted, whose peripheries were cut in exact copy of the curve produced by the synthetic curve machine. These curves were transmitted by vibration to the receiving diaphragm of a phonograph, and really formed an "automatic phonograph." With this instrument the sounds most resembling the vowel sounds of the human voice were easily recognised. But although the reproduction of vowels was good, it was imperfect, and it follows from this investigation, as far as it has gone, that vowels cannot be reproduced exactly by mechanical means. Something is always missing—probably the noises due to the rush of air through the teeth, and against the tongue and lips. One very curious result arising from the experiments with the automatic phonograph was to show that, by varying the pitch, the vowel sounds could be shifted,—*i.e.*, the curve which produced *oo* at a low velocity becomes approximately at *O* a higher velocity. *O* similarly becomes *ah*, *ah* becomes *a*, and *a*, *ee*. The curves arrived at synthetically do not differ very materially from those arrived at analytically by Helmholtz; they principally differ in the prominence of the prime. But curves produced by the synthetic machine, compounded of the different partials without their prime, show that there exist *beats* or resultant sounds. A vowel sound of the pitch of the prime may be produced by certain partials alone, without sounding the prime at all. The beat in fact becomes the prime. This point is clearly illustrated, orally, by the automatic phonograph, and graphically by the sketch drawn by the synthetic curve machine. The authors are investigating the true theory of the loudness of sound. They think that loudness does not depend upon amplitude of vibration only, but also upon the quantity of air put into vibration; and, therefore, there exists an absolutely physical unit in acoustics analogous to the unit of quantity of electricity or quantity of heat, and which may be called the quantity of sound. In commencing their research the authors endeavoured to find a disk for the phonograph and telephone which would vibrate to the finest shades of sonorous vibration. They obtained the best results from a stretched membrane of thin india-rubber, rendered rigid by a cone of paper.

March 6.—" Preliminary Report upon the Comatulæ of the *Challenger* Expedition," by P. Herbert Carpenter, M.A., Assistant Master at Eton College. The collection of *Comatulæ* made by the staff of the *Challenger* includes specimens from forty-five different localities, but few of which are deep-water stations. *Comatulæ* were only obtained seven times from depths exceeding 1000 fathoms. At lesser depths, 200 to 1000 fathoms, *Comatulæ* were met with at thirteen stations; but by far the greatest number, both of species and of individuals, were dredged at depths much less than 200 fathoms, and often less than 20 fathoms, at twenty-six widely-distant stations. At the present time the author regards the collection as containing 111 species, mostly new; but as the work of examination and description progresses it is not unlikely that forms which he now considers different may turn out to be merely local varieties of one and the same species, so that the number given above may be subject to alteration. Of these 111 species, 59 belong to the genus *Antedon*, 48 to *Actinometra*, 1 to *Ophiocrinus*, and 3, which are peculiar in having ten rays to the calyx instead of only five, to a new genus, for which he proposes the name *Promachocrinus* (Greek *Promachos*, " Challenger "). In two of the species the rays are undivided, as in *Ophiocrinus;* but in the third they divide, as in our common *Antedon rosacea*, so that there are twenty arms. The voyage of the *Challenger* has, according to the author, settled two curious questions in connexion with the Crinoids, the origin of which is due to Lovèn. They refer to *Hyponome Sarsii*, a so-called recent Cystid, and to *Phanogenia*, a supposed new genus of the Comatulidæ. *Hyponome* turns out to be nothing more than the disk of a *Comatula*, minus its skeleton. The stellate condition of the centrodorsal in *Phanogenia* has long been a puzzle to the author, but the material brought home throws a considerable light upon it. This condition appears to be one of the concluding stages of a long series of changes in the shape and relations of the centrodorsal, which do not commence until some time after the loss of the stem, and the entry upon the free state of existence. The dredgings in Torres Straits brought up a considerable number of specimens of a hitherto undescribed *Comatula*. The author proposes to name it *Actinometra Jukesii*. The examination of the *Challenger* Comatulæ has entirely confirmed the opinions held by Dr. Lütken and the author respecting the distinguishing characters of *Antedon* and

Actinometra. They both agree in referring forms with a (sub) central mouth, five equal ambulacra, and no terminal comb on the oral pinnules, to *Antedon*. On the other hand, species with an eccentric mouth, a variable number of unequal ambulacra, and a terminal comb to the oral pinnules, belong to *Actinometra*.

CHEMICAL SOCIETY, *February* 6. — Dr. J. H. Gladstone, F.R.S., President, in the chair.

Dr. Frankland opened a discussion on a paper read by Dr. C. M. Tidy on December 6th, 1878, "On the Processes for Determining the Organic Purity of Potable Waters." In the course of a long address Dr. Frankland said that he entirely dissented from Dr. Tidy's suggestion that non-volatile matter may be removed *mechanically* during evaporation. He had proved by actual experiments that mechanical removal only took place during violent agitation or the breaking of gas bubbles at the surface of the liquid. He was still more astonished, however, to find Dr. Tidy, who is a physician as well as a chemist, suggesting that the poisonous constituents of sewage may be volatile. All the knowledge we have hitherto acquired about the infectious matter of epidemic disease pointed, he said, to the inevitable conclusion that the propagating material is not merely organic, but organised; and that its virulence, unlike that of arsenic or other similar poisons, resides in its vitality and its power of multiplying itself almost indefinitely in the human body. An organised liquid, gas, or vapour, was a physical impossibility, and hence there is no foundation whatever for the opinion that the poison of sewage is volatile, or that it is not contained in the residue left on evaporation. With regard to the albuminoid ammonia process, Dr. Frankland's experience coincided with that of Dr. Tidy. It had been put forward as capable of answering the question, "Is the water wholesome or is it not?" It did not, however, answer this question: It condemned water containing only peaty matter, and it acquitted water containing urea and uric acid? Could anybody believe that in 1876 the Chelsea Company's water, drawn from the Thames, was "safe" in January, "dirty" in March and August, "safe" again in November, and "of extraordinary purity" in December, when it contained no less than 0·423 part of organic elements in 100,000 parts of water; and when three other samples drawn from the same source were denounced as "dirty" by the same process. Again in 1871, whilst the West Middlesex Company's water was "of extraordinary purity" in January, April, and May, the Chelsea Company's water was "dirty" in those months, although quite "safe" in December. Floods undoubtedly make Thames water dirty, but the albuminoid ammonia curve pursued the even tenor of its way quite regardless of them. Dr. Frankland cordially agreed with Dr. Tidy's strictures on superficial and rapid analyses of waters, in which the determinations were chosen with far greater regard to the analyst's time than to the needs of the inquiry. The so-called partial analyses were often very partial, and often brought disgrace upon the profession of chemistry. In conclusion, he congratulated the author on his valuable addition to the literature of water analysis, and on the clearness with which he had brought a great array of facts before the Society. His comparative tables were of great value, and he trusted they would be carefully studied by all chemists who practise water analysis. They formed a chart upon which the rocks so fatal to many a water analysis were so clearly shown that he who runs might read.

Mr. Wanklyn said that as long ago as 1867 Chapman, Smith, and himself directed attention to the cardinal defect of the combustion process, and this defect had never been overcome. It was this, that the organic matter in the water does not survive the evaporation to dryness. Dr. Frankland said that he would be content if he had a process to burn up the organic matter in the water itself. Messrs. Cooper and Wanklyn had invented such a process, and had burned up substances in solution.

Dr. Voelcker said that an impartial observer would come to the conclusion that all methods were more or less defective, and some gave very erroneous results. He would most earnestly urge the importance of determining all the constituents of a water, organic and inorganic, and not founding an opinion on one factor.

In his reply Prof. Tidy said he had concluded that the poison in water was volatile from analogy, because when it did act it was very virulent, and virulency and volatility usually go together.

EDINBURGH UNIVERSITY CHEMICAL SOCIETY, *January* 29.—Alexander Macfarlane, D.Sc., F.R.S.E., in the chair.

A paper was read by Mr. J. S. Thomson, "On Paraffin and what is got from it, as illustrated by 'Exhibits at the Paris Exhibition of 1878.'" A full account was given of the modes of preparation of these exhibits, which consisted of specimens of all the commercial products of the Addiewell Chemical Works. These included naphtha, burning oils, lubricating oils, solid paraffin in blocks (weighing not less than 7 cwts. each), sulphate of ammonia, and candles of various kinds made from the paraffin wax. The methods of preparation of other products of paraffin were also explained.

PHYSICAL SOCIETY, *February* 22, 1879.—Prof. W. G. Adams, President, in the chair.

Dr. Schuster gave the results of some observations made by a spectroscope with two prisms, one for the red and the other for the blue end of the spectrum, on the Spectrum of Lightning. Three observations were made, one at Las Ammas, one at Maniton, and one at Salt Lake City, last year. These showed the three nitrogen lines, with three well-defined bands and one doubtful band. The nitrogen lines correspond to the spectrum of air, and the bands appear to Dr. Schuster to agree with the spectrum of the light round the negative pole of the spark in a tube containing oxygen with adulteration of carbonic oxide.

Prof. Ayrton exhibited an Exisothermal Model of a Cooling Globe. If we imagine a globe initially heated throughout to a uniform temperature (as was probably the earth), and then kept in a space having a constant temperature, but much lower than that to which the globe was heated, then the temperature at every point of the ball will fall, but at very different rates; the parts, for example, near the surface cooling comparatively rapidly, while those near the centre will cool very slowly. A surface could, therefore, be constructed, such that the x of any point on this surface represented its distance from the centre of the globe, the y the time t from the commencement of the cooling, and the z the temperature of that point at that moment. The nature of this surface would depend on the size of the globe, on the specific heat, conductivity, and surface emissivity of the material. The "Experiments on the Heat Conductivity in Stone" of Profs. Ayrton and Perry, described in the "Philosophical Magazine" for April, 1878, enables them to determine these constants accurately for a trachyte sphere, and by using these data they have been enabled subsequently to construct such a surface, called by them an "exisothermal" one for a trachyte globe of 8000 miles in diameter, and which gives graphically the temperature of every single point of the earth from the moment when it was at the temperature of molten trachyte down to eight hundred thousand million years afterwards,—that is, until long after the present era.

March 8.—Prof. Ayrton brought forward a new theory of terrestrial magnetism, originated by himself and Prof. Perry, of the Imperial Engineering College, Japan. (See p. 287.)

Mr. F. D. Brown described his apparatus for maintaining constant temperatures and pressures. A constant temperature can be obtained if the pressure can be kept constant. The vessel in which the constant pressure is desired communicates with an air-pump by a pipe in which a movable tap or valve is placed. By opening or closing this tap the pressure is regulated. This is effected by an electric clutch arrangement. A mercury anemometer sends a positive or negative current from a battery through the clutch according as the pressure is too high or low, and this current actuates the clutch to close or open the valve. The clutch consists of an axle driven by a turbine to get power to work the valve, and the current, by means of electro-magnetism, connects the tap to the axle, which then opens or closes it as the case may be. In this way a pressure varying no more than one-fifth millimetre each way can be obtained.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY, *February* 18.—J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the chair.

Professor Roscoe, LL.D., F.R.S., communicated a paper by Sadamu Ishimatsu, "On a Chemical Investigation of Japanese Lacquer, or 'Urushi.'" During a few months last year the author had the opportunity of examining roughly into the nature of "Urushi," in the Laboratory of Tokio University. The specimen of lacquer which he had under his examination was obtained from Kuyemon Nakamuraya, in Tokio, a large lacquer merchant. It is a milky juice of pale grey colour, and gives out a certain kind of poisonous volatile gas. Some persons are terribly attacked by this poison, producing a great swelling where the acid comes in contact. During the author's examination in the laboratory one of the apparatus keepers was terribly attacked by this gas, producing ugly swellings all over the face. By using the solution of chloride of sodium, carbonate of soda, acetate of lead, &c., he was said to have recovered within a week. This poison, however, acts only on certain persons, for the author while working with it never felt any uneasiness from it. The blackening of lacquer in air is by many supposed to be due to the combined action of light and air, but from the following experiments this seems to be erroneous:—1. A square box was made which had a well-fitting sliding door, and the inside of which was made perfectly black, so that practically no light was permitted to enter. In it was placed a small quantity of lacquer at dark, and the door closely shut. On looking at it the next morning it was observed that the lacquer had turned perfectly black, proving that it is not light that blackens the lacquer. 2. The bottle in which the specimen of lacquer was kept for more than three months was exposed to the incident light of the laboratory. The surface of the lacquer was turned perfectly black, forming a wall as it were; while those portions which were in contact with the sides of the bottle, which receive as much light as if there were not any glass sides before it, were not at all blackened. This phenomenon proves that the blackening in the atmosphere is in all probability due to the oxygen of the air, but not to light alone, nor to the combined action of light and air. The examination showed that the "Urushi" consists of three principal constituents: (1) a resinous part soluble in alcohol, (2) gum, and (3) residue. The resinous part soluble in alcohol seems to be the principal portion: it has a smell like ordinary lacquer, but it never dries as the original does. It is brownish black, and slightly sticky to the touch. When treated with potash solution it forms a bluish black precipitate, but nothing is obtained on addition of sulphuric acid to the filtrate. When boiled with hydrochloric acid the resin is transformed into a substance elastic while hot, something like the mass obtained when heated sulphur is dropped into cold water. When boiled with nitric acid nitrous fumes are given out, and the mass gradually becomes yellow, and finally a beautiful orange-coloured mass was obtained. The mass was to a great extent soluble in absolute alcohol, leaving behind a small quantity of a yellowish body. A quantity of alcoholic extract was precipitated with acetate of lead, the precipitate was thoroughly washed with absolute alcohol, aad then decomposed by means of dilute sulphuric acid. The mass was dissolved again in absolute alcohol, then separated from sulphate of lead. This separated alcoholic extract was again precipitated by sugar of lead, and, after filtering and washing, the precipitate was dried partially in an air-bath, and carried under the receiver of an air-pump and dried over sulphuric acid. This lead salt exploded when heated. The amount of lead was estimated as oxide by igniting it with nitric acid, and the salt was subjected to organic combustion. Nitrogen was determined by Dumas's method. The following numbers were obtained as the mean results :—

Carbon	26·93
Hydrogen	4·11
NO_2	18·44
PbO	47·42
Oxygen	3·10
	100·00

The author then took alcoholic extract of the original lacquer and precipitated it with acetate of lead. After requisite purification and drying the precipitate, it was analysed, the lead being determined as before. The following is the mean of two experiments which were tried :—

Carbon	50·450
Hydrogen	5·705
PbO	3·775
Oxygen	40·070
	100·000

The gum is soluble in cold as well as in warm water. It has no smell, almost no taste ; it has a yellowish, or rather brownish, colour, and is of a non-crystalline body. It is quite insoluble in alcohol. On subjecting this substance to organic analysis the following percentage of hydrogen, carbon, and oxygen was obtained :—

Carbon	41·20	41·45
Hydrogen	6·51	6·58
Oxygen	52·29	51·97
	100·00	100·00

These analyses yield a formula approximating to the composition of common gum. The residue is, the author thinks, nothing more than the mixture of cellulose, bark, dust, &c.

NOTES.

AGRICULTURE.

FROM a paper by A. Johnen, in "Biedermann's Central-blatt" for January, 1879, "On Comparative Observations of Rainfall according to Fautrat's Method," we learn that the quantity of rain in a beech-wood is 13 per cent greater than in the open country. In a pine-wood the excess was only 2 per cent.

From manure experiments on the Agricultural Trial-field of the University of Giessen, Prof. A. Thaer finds that the quantity of nitrogen given to a selected "morgen" of land in the form of manures during seven years amounted to 89·5 lbs. The crops reared during the same years contained together 198·69 lbs. of nitrogen. Consequently the soil had given 109·19 lbs. nitrogen more than it had received. Hence, while the manures furnished 45 per cent of the nitrogen in the crops, the atmosphere had yielded 55 per cent.

According to Dr. E. Wein, Prof. J. Nessler, Prof. A. Mayer, and Prof. R. Heinrichs, the composition of an English cattle food is—

	Wein.	Nessler.	Mayer.
Moisture...	13·72	—	12·9
Albuminoids	11·75	13·21	9·4
Oily matter	4·28	4·24	3·6
Non-azotised extractive	67·24	—	66·9
Vegetable fibre ...		—	3·6
Ash	3·01	—	3·6

Wein and Mayer, from chemical and microscopical evidence, conclude that the food is composed of equal parts of bruised maize and locust-beans, and that the essential oil of a plant of the umbelliferous order has been added. Another English cattle food in the form of cake contained also a large proportion of locust-beans.

BIOLOGY.

Mr. H. W. Bates, in his Presidential Address to the Entomological Society, refers to some unexpected facts in the geographical distribution of insects. Thus the insect fauna of New Zealand is quite distinct from that of Australia, and its origin will therefore have to be accounted for by a totally distinct set of causes. The Carabidæ of Australia have often a near but very peculiar relationship to forms of the same family in the Palæarctic region. Next to that great region, indeed, Australia is the richest part of the world in species and genera of the

Feronia group—a circumstance which ten years ago would have been deemed most unlikely.

Mr. J. W. Slater, writing in the "Entomologist," shows that the larvæ of gnats, midges, &c., instead of being sanitary agents, intensify the putrefaction of organic refuse suspended in water.

At a recent meeting of the Biological Society of Paris, M. Bert described the results of his experiments on the temperature of the brain. In a state of absolute inaction, bodily and mental, the temperature of both sides of the head is equal; but as soon as any brain-work is undertaken, if the equality does not continue, the left side always shows the higher temperature. According to M. Raymond, in cases of paralysis the temperature is higher in the parietal region than over the rest of the cranium; and here, again, the left side is found hotter than the right by 0·3 to 0·4 of a degree C.

At another meeting of this Society (March 1st) Dr. P. Bert described experiments made to elucidate the influence of the cerebral hemispheres upon the capillary circulation. One hemisphere of the brain of two axolots, a black and an albino respectively, and also of a bat, was removed. No modification of the capillary circulation resulted. There was produced, however, a bilateral paralysis of the chromotaphores of the skin, and the animal lost the power of changing its colour under the influence of emotions.

At a meeting of the Bordeaux Société des Sciences Physiques et Naturelles, Dr. Azam gave an account of an interesting case of double consciousness and scission of personality which has been under his observation for many years, and which has been described in the "Comptes Rendus de l'Academie des Sciences Morales." The patient, Felida X——, has, as it were, two distinct lives, two conditions separated from each other by a short sleep or a torpor of a few seconds. After her return to the normal state she is entirely unconscious of what has taken place during the abnormal condition, whatever its duration. She is more cheerful during this abnormal state than in the normal condition. When in the abnormal state she remembers, however, anything which has happened in her normal intervals. The author, in accordance with M. de Quatrefages, considers that the mnemonic cellules are disseminated throughout the brain. He formerly thought that they were seated in the right hemisphere, because the faculty of articulate speech, which resides in the left hemisphere, is never affected in Felida. This opinion is difficult to maintain, since the faculty of articulate speech cannot act without memory.

From a paper read before the Shanghai branch of the Royal Asiatic Society, by M. A. A. Fauvel, it appears that a true alligator—as distinct from the crocodile—has been found in China. Hitherto the alligator has been supposed peculiar to the Western

Hemisphere. The name proposed for the species is *Alligator Sinensis*.

Dr. A. Voisin finds that the brains of idiots are structurally little developed, remaining in a fœtal condition, or at most in a state like that of early childhood. The convolutions are abnormally simple, and present only a few nucleoli.

Mr. W. H. Edwards, in the "Canadian Entomologist," records the singular fact that ants cherish the caterpillar of *Lycæna pseudargiolus* (a small butterfly closely resembling the "blues" of English collectors), for the sake of a clear, green fluid, doubtless of a saccharine nature, which exudes from a peculiar mamilloid organ on the back of the eleventh segment, The most perfect understanding seems to prevail between the ants and the caterpillars, which must thus rank with the *Aphides* among the domestic cattle kept by our six-footed rivals.

CHEMISTRY AND TECHNOLOGY.

In a note on large crystal growing Mr. C. W. Quin remarks that a great difficulty is to guard against sudden rises in temperature, which generally have the effect of causing the growing crystal to be partially and unevenly re-dissolved. To receive a continually even temperature, after many experiments he hit upon the plan of plunging the beaker containing the growing crystal into the house-cistern, the temperature of the water in which never differed by more than 0·5° F. either way day or night. To prevent the solution from becoming exhausted too rapidly he immersed in it to the depth of half an inch or so a crystal drainer containing a filter-paper full of the salt to be crystallised, so that a constant stream of strong solution was continually descending on the growing crystal.

The double staining of vegetable tissues, various processes for which have been noted under "Microscopy" from time to time in the "Journal of Science," has proved eminently successful in rendering structural details more evident. Those portions consisting of tissues hardened by much secondary deposit absorb the dye with avidity, and in the washing process part with it but slowly, so that the simpler tissues are deprived entirely of their colour before the hard parts yield up theirs; careful staining with another dye follows, and the effects of differential colouring are very apparent. Some operators have used three and four colours with great success.

M. G. de Plessis* recommends bichromate of potash for the preservation of delicate marine organisms, such as polypes, *Medusæ*, *Salpæ*, &c. The solution can be made in either fresh or salt water, and a weak solution answers well, exact proportions being of but little moment. A specimen of *Medusa aurita* has kept well a whole year, and its beauty and transparency leave nothing to be desired. The solution has only one inconvenience, it permits the development of mould; but this can be prevented by the addition of a few drops of phenic acid or phenic alcohol. Another convenient salt is permanganate of potash, acting very much like osmic acid, clearing away the protoplasm and bringing out the minutiæ and showing nuclei, outlines of cells, &c. It can be dissolved in sea-water, and should be used as a saturated solution either in sea or fresh water. This solution kills small organisms at once. They are left in it from thirty minutes to an hour, then withdrawn and placed in alcohol, after which they can be made transparent with turpentine and mounted in Canada balsam. According to the author it effectually replaces the costly and somewhat dangerous osmic acid.

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* Bulletin de la Société Vaudoise des Sciences Naturelles, sér 2, vol. xv., pp. 278—280, April, 1878.

We learn from M. Reimann's "Färber Zeitung" that two new green dyes have been introduced into practical use; the *Vert acide* of A. Poirrier, of Paris, and the Victoria green of the Baden Aniline Company. The price of the latter is only 16s. per kilo.

The red azo-colouring matters are fast superseding cochineal in the flannel dye-works of Saxony. They have the advantage of not being injured by washing.

M. Aimé Girard has reported to the Société d'Encouragement pour l'Industrie Nationale on M. Kuhlmann's method for recognising the composition and measuring the volume of the gases and acid vapours given off by the chimneys of chemical works. By means of this arrangement a manufacturer can at any hour inform himself concerning the nature of the gases traversing his chimney. To establish the composition of the chimney gases and to detect, *e.g.*, the presence of hydrochloric acid therein, M. Kuhlmann introduces into the shaft a glass tube which communicates with a series of test-tubes, at the end of which is a cistern of water which acts as a respirator. The flow of water compels the gas to traverse the series of test-tubes, the first of which contains caustic soda coloured with litmus. Then follow solutions of barium chloride and silver nitrate. The dimensions of the cistern are such that five or six hours are required for the total efflux of the water which it contains, and the strength of the alkaline liquid coloured blue is such that in the regular course of work five or six hours are required to turn them red. An earlier colouration gives immediate warning. M. Kuhlmann's method of measuring the total volume of gases escaping is as follows:—When he wishes to know the speed of the gases he introduces at the bottom of the chimney a certain volume of a strongly coloured gas, such as hyponitric acid, which accompanies the colourless gases up the chimney and presents itself along with them at the top. Knowing then the volume of the chimney and the mean temperature it is possible to calculate from the time which has elapsed the quantity of gas emitted.

Artificial benzoic acid is at present manufactured in large quantities from the naphthalin that is abundantly found as a by-product in the gas works.

The efficiency of creosote as a preservative of timber against the ravages of the teredo has, remarks the "Engineering and Mining Journal," been signally established by the recently-published report of a commission appointed by the Royal Academy of Sciences of Amsterdam to make a thorough investigation of the subject. This commission selected several of the ports of Holland in which to conduct their experimental trials, and subjected to their tests oak, red-fir, common fir, and pine, in pieces about 3 feet long and 12 inches square. They found that all external applications, such as coal-tar, paraffin varnish, mixtures of tar, resin, sulphur, &c., were absolutely worthless. Mechanical protection, by the use of nails, was found to be only partially efficacious, and, with the use of copper or iron sheathing, too expensive for ordinary use. Impregnation with various materials was also fully experimented upon, and, as against this element of destruction, with the single exception of creosote oil, all proved to be either completely or almost worthless. In this category are named sulphate of copper, copperas, acetate of lead, salts of mercury and arsenic, soluble glass, chloride of calcium, and paraffin oil. Only the blocks treated with creosote oil were found at the end of two years to have remained uninjured. The commission thereupon draw the conclusion that the only effective preservative agent for timber against the depredations of the marine boring-worms is creosote properly applied and of good quality.

According to C. Fürstenau in the "Chemiker Zeitung" the Austrian ultramarine manufacturers sophisticate their colours with three parts of gypsum, and lest the colour should appear too pale it is further mixed with glycerin or glucose or a mixture of both. This keeps the powder damp and renders the colour apparently deeper.

Great industrial activity is being displayed by the German Distiller's Association. At a general meeting it was resolved to establish and maintain a chemical laboratory, an experimental distillery, a school of distilling, a trade journal, a glass-blowing establishment for the manufacture of normal hydrometers, and an office for general intelligence.

It is stated in "La Lancette Belge" that Mr. Edison has invented an ink which gives raised characters upon paper capable of being read by the blind by touch.

The formation of mannite in beer is always a mark of the want of cleanliness and is particularly promoted by decaying wood. In such cases the sugar present enters not into the alcoholic but the mucic fermentation.

Dr. Upmann, in "Chemiker Zeitung," in consequence of some experiments pronounces oxalic acid to have a harmless effect on dogs. Dr. Pfeiffer, in reply, points out that the stomach and intestines of dogs generally contain considerable quantities of calcium phosphate by which the oxalic acid would be converted into calcium oxalate, an inoffensive salt.

GEOLOGY.

M. W. H. Hermite, in a paper read before the Academy of Sciences, concludes that the form of the earth is due not to a previous condition of igneous fluidity, but mainly to the configuration of its seas. The remarkable polar flattening of Mars is due, according to his hypothesis, to the different distribution of land and water upon the surface of that planet. Volcanic phenomena he finds also incapable of being referred to the existence of a central fire, or even to the presence of a sea of lava of small extent.

The First Part of Vol. vii. of the "Proceedings of the Yorkshire Geological and Polytechnic Society" contains a number of useful papers. Mr. Sorby gives an account of his new method of studying the optical characters of crystals by looking through them with a microscope at a circular hole or rectangular grating. The Rev. J. F. Blake communicates a paper on the geological history of East Yorkshire. The Rev. E. M. Cole describes the red chalk of the East Riding, Lincolnshire, and Norfolk, which lies at the base of the white chalk and is free from flints. Mr. J. W. Davis, the indefatigable Secretary of the Society and Editor of the "Proceedings," contributes an account of the fish-remains found in the coal measures and the evidence which they afford of fresh-water origin. He shows that the coal was deposited in a series of pools, into which the vegetable matter was carried whose decayed remains form the coal, and when mixed with the mud simultaneously brought down the "hubb," or black bituminous shale. In this, as well as in the coal, large numbers of Cœlacanths have been discovered, as well as Ganoids and Elasmobranchs, though much more sparingly.

We learn from the "Engineering and Mining Journal" that a resolution of thanks to Prof. Hayden for his "accurate and comprehensive survey of the State of Colorado," passed the Legislature of Colorado on January 14th, and upon which Senator Gaussion remarked: "These reports, coming from a scientific and authoritative source, do more to answer and satisfy the inquiries of capitalists than every thing else. They tell the world what the great Western country is made of. The Western domain of the United States is, to-day, the glory of the nation."

METALLURGY.

In a paper "On the Relations between the Chemical Composition and the Mechanical Properties of Steels" (Bull. de la Soc

Chem. de Paris), M. J. Deshayes says that carbon renders steels rigid and elastic, increasing their elastic tension, but their resistance to rupture diminishes if 0·500 is exceeded. Manganese renders steels rigid and elastic, and increases their elastic tension, but the elongation and contraction remain considerable, which gives a good resistance to a shock. Silicon plays the same part as carbon, rendering steels hard, and slightly diminishing elongation. Sulphur decreases the breaking strain and the resistance to a shock. Phosphorus renders steels deficient in body, and, if its proportion exceeds 0·250 per cent, fragile on receiving a shock. Chrome acts like manganese, but more energetically.

According to the "Revue Industrielle" a metal has been extensively produced in France which is a combination of iron and steel. The two metals are run separately into a mould divided into two parts by a thin plate of sheet iron, when the whole becomes welded together. The new metal is particularly adapted for armour-plating, anchors, and safes.

PHYSICS.

The Committee appointed to receive subscriptions to present a bust of Mr. William Spottiswoode, Pres. R.S., to the Royal Institution as a testimonial of his valuable services as its Treasurer and Secretary successively, have engaged Mr. Richard Belt as the sculptor.

On behalf of the Committee of Economical Arts, M. le Compte du Moncel has presented to the Société d'Encouragement pour l'Industrie Nationale a report on M. E. Regnier's electric light regulator. The Committee decide that M. Regnier has solved the problem in question. His arrangement has been successfully tried at the station of the Northern Railway, and in M. Breguet's workshop, where it has acted for some hours with regularity under the influence of the current of a small Gramme machine.

A remarkable case of cohesion, or the welding of two metals at a temperature far below the melting-point of either of them, has lately been noticed by Mr. Charles A. Fawsett, of Glasgow, and by him reported to Sir William Thomson. If a piece of silver, 1 centimetre square, is heated on the inverted lid of a porcelain crucible, to about the temperature of 500° C. (932° F.), and the end of a thin platinum wire is brought into contact with it, the two metals will be found to have welded to that extent that the silver may be raised from the lid, and will remain attached to the platinum wire when cooled off. Mr. Fawsett reports that other metals—copper and aluminium, for example—will likewise adhere to silver, though the experiment is less striking than in the case of platinum.